STUDIES FROM THE PSYCHOLOGICAL LABORATORY

OF THE

UNIVERSITY OF CALIFORNIA

II

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Study

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BY PROFESSOR GEORGE M. STRATTON.

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STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF CALIFORNIA.

I. A NEW DETERMINATION OF THE MINIMUM VISIBLE AND ITS BEARING ON LOCALIZATION AND BINOCULAR DEPTH.

BY PROFESSOR GEORGE M. STRATTON.

The smallest lateral difference of place that is visible has until recently been given as about 50″–60″ angular measure. The method employed by Helmholtz and others[1] in reaching this result was the well-known one of bringing two parallel lines together until they finally are just distinguished as *two*—on the same general principle by which Weber determined the tactile space-threshold, by finding the distance between two compass-points that just cease to merge into *one*.

But by a different method it is now evident that a lateral difference[2] of place of about 7″ of arc can be directly perceived.

Instead of using lines or points side-by-side, the experiments which give this result were made with lines end-to-end, so arranged that the upper of two perpendiculars could be moved at will to the right or left while still remaining exactly parallel to the lower line, as shown in Fig. 1. The observer had simply to judge whether the upper line was continuous with the lower, or to which side it had been displaced. In the initial trials the lines were narrow slits of light surrounded by

[1] See Helmholtz: *Physiologische Optik*, 2d ed., pp. 256 et seq.

[2] In contrast with a difference in depth or a difference not of itself perceptible, but evident merely by reason of stereoscopic effect.

a black ground—each line not more than half a millimeter wide and some 10 cm. long, and observed in a half-darkened room from a distance of 18 meters. But the surprising fineness of discrimination made it impossible to work with accuracy from so short a distance; so that finally the observer was placed 120 meters away, and the lines were changed to narrow white strips, part of the time 10 mm., part of the time 8 mm., wide, each 50 cm. long, on a dull-black ground. Each line with its ground was mounted on the face of a plate of glass, the two glasses placed edge to edge in a frame; and, by sliding the upper plate upon the lower, lateral shifts were made by steps of 1 mm. At first the ordinary procedure of 'minimal changes' was adopted, but it soon became clear that suggestion was playing too large a rôle, and resort was had to a compromise between this method and that of 'right-and-wrong cases,' which might be called a method of serial groups, to be described more fully at another time. Taking as the threshold the point where 80%, or more, of correct judgments occurred, the following results were obtained:

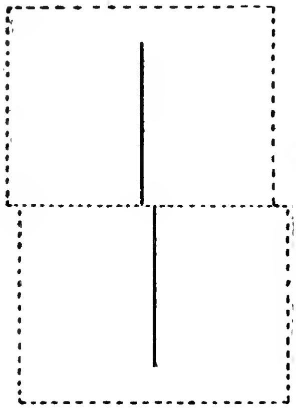

FIG. 1.

SUBJECT A. (187 judgments).

THRESHOLDS FOR DISPLACEMENTS TO THE

	Right.	Left.
	2. mm.	5. mm.
	4. "	6. "
	3. "	5. "
Aver.	3. mm.	5.3 mm.

General average 4.1 mm.

SUBJECT D. (277 judgments).

THRESHOLDS FOR DISPLACEMENTS TO THE

	Right	Left.
	4. mm.	6. mm.
	3. "	5. "
	2. "	5. "
	4. "	5. "
Aver.	3.2 "	5.2 "

General average 4.2 mm.

Taking 1 mm. as equivalent, approximately, to 1.7 seconds of arc, where the radius is 120 m., we get a fraction over 7 seconds as the threshold of space-distinction under these conditions.

The experiments were begun with some doubt whether there was a direct perception of a *spatial* difference here; whether, for instance, some purely intensive change—some apparent dimming or strengthening of the impression from the adjacent ends of the lines where the displacement occurred—might not serve to suggest indirectly a spatial inequality. But anyone making the observation is soon convinced that what he sees is not of this character, but that the two lines seem to form a single line no longer straight. He seems to compare the position of one of the lines with an imaginary extension of the other, and to notice that the two, in this way, do not coincide. As a check, however, the observers throughout the experiments were kept in ignorance of the actual direction of the displacements, and the threshold was not considered as reached until the direction could be told by them; so that if their judgments had been based on anything other than the perceptible position of the lines, the fact of a break or shift in general might perhaps have been inferred, from some intensive difference in the light, but it is difficult to see how, from that alone, the subjects would have been able to tell correctly to which side the line had been moved. There seems, moreover, to be nothing inherently suspicious in the striking disparity between these results and those obtained by the older method. In the threshold obtained in Helmholtz's way these finer measurements are not reached simply because the diffusion of the stimulus from the two lines side-by-side makes a fairly uniform blur on the retina between them, and within the limits of the blur two separate objects can no longer be distinguished. But when the lines are arranged as in the present experiment, any such interference by mere diffusion is greatly lessened, and the localities can be clearly and correctly distinguished.

The result thus obtained is interesting in several ways. In the first place, it probably removes the grounds for inferring, as yet, that stereoscopic depth at its minimum is a *subconscious* result of the spatial conflict of the two images. My own ex-

periments with the pseudoscope[1] had shown that an angular difference of 24 seconds between the two impressions was sufficient to give a binocular relief. Still later, Bourdon,[2] experimenting with needles at short range, found that a difference amounting to but five seconds produces a perceptible depth-effect. As long as the conscious lateral threshold was counted as above 60 seconds, one would be tempted from these results to believe either that stereoscopic depth was not due primarily to a lateral space-discrimination of the two images, or else that the plastic effect must depend on a subconscious action, since the disparities in the image were smaller than could be consciously noted. The present reduction of the conscious lateral threshold to about 7 seconds leaves but a small margin upon which to base such conclusions, particularly when one takes into account the wide contrasts in the (non-essential) conditions of the experiments. Bourdon would seem entirely justified, however, in concluding that binocular depth cannot be due to our detecting double images, if by double images we are to understand outlines that are distinguishable *side-by-side*. But the depth-effect may still be due to the presence of double images in the sense of outlines that are felt not to be coincident when positions are compared *end-to-end*. There certainly is a difficulty in that ordinary stereoscopic vision seems hardly to provide the conditions for comparing outlines in this way. It is barely possible, however, that the curious phenomenon of retinal rivalry may be useful just herein, that by the successive emerging and disappearance of parts of the outlines in the two projected fields of view something comparable to the conditions of the present experiment is brought about, and an exceedingly fine perception of lateral incongruity results.

But perhaps a more important bearing of the experiment is on the general problem of visual localization. It seems highly improbable that so minute a displacement is discerned by noting some muscular jog or unevenness in running our eyes up and down the line, when one recalls that the fovea itself is some

[1] A Mirror Pseudoscope and the Limit of Visible Depth, PSYCHOLOGICAL REVIEW, Vol. V., p. 632.

[2] L'acuité stéréoscopique, *Revue Philosophique*, January, 1900.

300 times broader than the retinal image of the space-inequality here perceptible. In view of this relatively wide expanse of the fovea, it seems highly questionable whether the eye, in running up and down such a pair of lines would regularly take one course when the lines exactly met and a perceptibly different course when one line was displaced 7″. The eye naturally moves by twitches and jerks, even when following a straight line; the breadth of the fovea is such as to permit considerable roving without 'losing' the line. So that so small a dislocation in the objective line would probably be no incentive to an exactly corresponding change in the movement of the eye. And even supposing that a dislocation of 7″ in one of the lines did regularly tend to draw the eye by so much out of its course; how should we be able accurately to interpret so slight a variation of muscular action, as clearly due to an objective spatial inequality, when much greater movements—likewise involuntary, are constantly occurring without our interpreting them as due to a spatial variation in the object we are observing?

If, on the other hand, we pass from the muscular apparatus and look to the minute elements in the retina to explain such discriminations, even these seem much too gross to account for the marvellous fineness of our judgment. Rows of cones in the mosaic of the fovea lie apart a distance corresponding to at least an angular measurement of 30″. To explain our power to detect a spatial difference one-fourth of this, it will be necessary to assume either that the rods and cones are not the ultimate spatial elements in the retina, or else that the *minimum visible* may be considerably less than the distance between the center of the adjacent sensory elements.

And, after all, it is not difficult to see how this latter might well be the case. We must, however, first give up the notion that the light, even when it falls upon a single cone, or row of cones, affects only those elements upon which it directly falls. In every case the stimulation is probably diffused in all directions; a responsive wave runs through the neighboring elements, as is evidenced, for instance, by the fact of simultaneous contrast. Now if C and C' be adjacent elements, and b the boundary between them, then it seems not improbable that if a ray

fall somewhere within the limits of C its effect upon C' will be different according as its point of incidence is nearer or farther from b. It would probably excite C' more intensely the nearer it fell to the limits of this cone; and, on the other hand, its effect upon C'', a neighbor on the opposite side of C, would correspondingly decline. In such an event there would be a difference of nervous result with every change, however small, of the point of incidence, instead of a change occurring only in case the stimulus passed to an entirely different cone. There would then be a difference of intensity of the diffusive effect upon neighboring elements, a different degree of whatever kind of reaction may be characteristic of each, and consequently a change of 'local sign' even when the difference of place of the excitation is considerably less than the diameter of a single element. The local signs in the retina alone, quite apart from the muscular mechanism, would thus form an absolutely continuous series, and would furnish the data for any degree of spatial discrimination we may discover. The retinal sign of position is thus conceived, not often the analogy of our electric signal boxes, when one unalterable mark is given (a falling numeral, for example) whenever the same terminal is affected; but we should have to symbolize it rather by some more complicated contrivance: where nothing less than several neighboring numbers dropped on each occasion, but each of these appeared with differing clearness according as its particular terminal was near or far from the immediate origin of the disturbance. The relation of the various intensities could in this case be a sign both of direction and of distance; and the exact seat of the exciting cause be determined with a degree of accuracy depending on the fineness of discrimination for intensive differences and for catching their interrelation, rather than upon the number and distance apart of the several terminals; in other words, the threshold would not depend on the purely anatomical meas-

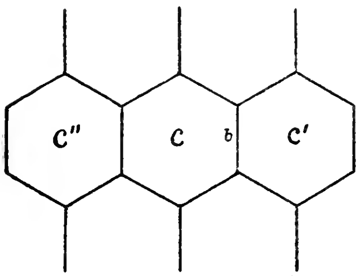

FIG. 2.

urements, if we may drop the figure and return to the retinal fact.

The present experiment, then, tempts one to believe that the local signs are of this exceedingly complicated character. The mental process of localization, or of space-distinction, cannot be justly described (it would seem) as an association merely between a particular quality of sensation and a particular place. Nor is it entirely sufficient to amend this and say that the mind must also take account of the various intensities of the quality which is spatially significant. The complication seems to go still a degree higher, so that the *interrelation* of numberless intensities of different retinal sensations would seem to be the intricate process involved in even the simplest visual perception of space. When we bear in mind that the fully organized perception without doubt includes also extra-retinal data, it is evident how complex an activity our spatial consciousness is.

II. The Effect of Imperceptible Shadows on the Judgment of Distance.

BY KNIGHT DUNLAP.

The conscious effect of stimulation of such low intensity as to be imperceptible presents an attractive and almost unexploited field for experimental work. The experiments of Jastrow and Pierce on small differences of sensation,[1] and some previous work of my own, suggested the possibility of obtaining important results from an experiment planned to show directly the effects of the presence or absence of an imperceptible stimulation.

The Müller-Lyer figure was selected as the foundation for such an experiment. If we have the segments of the principal line in the illusion-figure distinctly marked, but the angular lines of an intensity just below the threshold of perception, we have the simple materials for determining whether or not these imperceptible lines will produce in any degree the ordinary illusion-effect. If such an effect is produced, then we have

[1] Memoirs of the National Academy of Sciences, Vol. III., p. 76.

evidence for the belief that under certain conditions things of which we are not, and can not become, conscious have their immediate effects upon consciousness.

The apparatus used in the first investigations under these conditions was very simple, and not altogether satisfactory in its operation, but as the experiments were of the nature of a preliminary survey of the ground, it was not deemed advisable to make a complicated arrangement of apparatus.

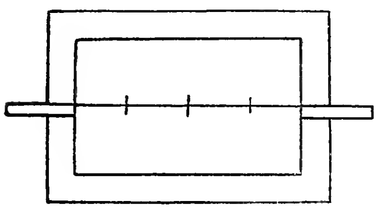

FIG. 1.

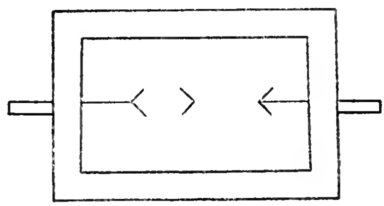

FIG. 2.

The screen upon which the illusion-figure was to be shown was a sheet of white bristol-board, fifty-eight centimeters by seventy-two centimeters, fastened upon a frame. A black line one millimeter in width (see Fig. 1) was drawn across the middle of the screen horizontally, and divided in the center by a perpendicular line three-fourths of a millimeter in width, and extending eight millimeters on each side. On the sides of the frame holding the screen, and on a level with the horizontal line were fixed guides, in which slid small steel rods passing along the line and hidden against it. Small strips of black paper two millimeters wide and ten millimeters long were fastened to the inner ends of these rods in positions parallel to the central vertical line and extending equally above and below the horizontal line, and these with the central vertical line marked off two segments of the horizontal line. The lengths of these segments could be varied at will by sliding the rods along the horizontal line.

Fastened to each of these rods carrying strips, and parallel to them, was another exactly similar rod passing behind the screen and bearing on its inner extremity an angle of ninety degrees, cut from black bristol-board, with legs five millimeters wide and

forty-five millimeters long. These angles, together with a similar one fastened in the center, showed upon the front of the screen as shadows when light was transmitted through from the back, and transformed the segments of the line into the typical Müller-Lyer figure. The arrangement of this part of the apparatus may be understoood from Figs. 1 and 2, which show respectively the front and back of the screen. It will be observed that by interchanging the pairs of rods and reversing the central angle the direction of the illusion could be reversed.

The screen was illuminated from the front by two hooded incandescent lights placed one at each side so as not to obstruct the view, and at equal distances from the screen. Behind the screen was a single hooded incandescent light, so shielded with tissue paper as to diffuse the light as evenly as possible over the back of the screen. The intensity of this light, and therefore the intensity of the shadows on the front of the screen, was controlled by means of a rheostat, the front lights remaining unchanged. As the current for the three lights was taken from the same circuit, variation in the potential affected them all in approximately the same ratio, and hence the relative intensity of the light coming through the screen to the light falling upon the face of it, as determined by the adjustment of the rheostat, might be supposed to remain fairly constant.

The method of operation was very simple. One of two figures, a double square and a circle, was placed against the back of the screen to test the intensity of the light, and the intensity of the rear light was reduced until the subject was unable to detect the shadows caused by the figure. Upon reaching a point at which it was certain that the subject could not tell whether the figure was circular or square, it was removed and the angles of the Müller-Lyer figure placed in position. It was determined by the toss of a coin whether the angles should be set in the 'short' or 'long' position,[1] and the left cross-line was fixed at twenty-five centimeters from the center. The right

[1] The direction of the angles which tends to shorten the left or standard segment of the line, thus <—>——<, will, throughout, be spoken of as 'illusion short.' The opposite direction of the angles, which tends to lengthen the left segment, will be spoken of as 'illusion long.'

cross-line was started from a point sufficiently greater or less than twenty-five centimeters from the center to be distinctly perceived as farther or nearer than the left one, and moved at regularly timed intervals[1] by steps of one millimeter towards and past the equality point, the judgment of the subject as to the length of the right segment of the line as compared with the left segment being recorded at every step in the series. Whether the cross-line should move inward from a point beyond the equality point, or outward from a point inside, was determined by lot, correction being made toward the close, however, so as to have on the whole as many series of one kind as of the other in order to offset the effect of mere direction of motion. It is evident from the details given above that the angle behind the right cross-line moved with it, so that the relations of the Müller-Lyer figure were constantly preserved.

Series of this kind alone would of course not be sufficient to determine whether or not the illusion figure is effective. As will be seen later, there is a tendency to judge the equal segments of a line as different even apart from any influence of the angles of the Müller-Lyer figure. Hence it was necessary for purposes of comparison with these to take also series in which there could be no possible illusion, since the light behind the screen was entirely cut off. If there should be any effect produced by the shadows under the conditions previously stated, a comparison of the series with those taken when the shadows of the angles were present but imperceptible might show it.

The series were consequently taken in pairs or sets, each pair being composed of one series with the shadows, and one without, taken in immediate succession, in the same direction, and from the same point. The order in which the two were taken was determined for each pair by lot, as was also the direction of the illusion as mentioned above. The subject being ignorant of the results of the lots, there was a double check upon the possibility of any influence arising from his knowledge of how the illusion might be expected to affect his judgments. Any general difference between the two classes of series could

[1] The interval was nine seconds in length from the completion of a judgment until the command to look again at the screen.

therefore only be due to the effect of the angles behind the screen.

In general, each series proceeding outward resulted in first a number of judgments of 'shorter,' then one or more of 'equal' or 'doubtful,' and finally, a number of 'longer.' In the series proceeding inward the order was reversed. The middle point of the region of doubt and equality was taken as the mean equality point of each series, and this was compared with the mean equality point of the other member of the pair. The region of doubt and equality was determined by fixed rules, and, in order to secure absolute impartiality the point was determined without the experimenter himself knowing to which class the series belonged. For this purpose a number of the records were allowed to accumulate, were shuffled, and their distinctive marks concealed until their mean equality points were determined and recorded.

Three subjects were employed, and the results of the work with them are summed up in Table I. A set was counted 'for' or 'against' the illusion according as the difference between the mean equality points of the two series composing it was or was not in the direction which would correspond to the possible effect of the illusion-figure.

TABLE I.

Subject.	Total Pairs.	For Illusion.	Against.	Neutral.
A.	23	14	8	1
R.	11	9	2	0
S.	13	8	5	0

We see from this table that 60 per cent. of the sets for subject A. fall on the side of the illusion, 81.8 per cent. for R., and 61.5 per cent. for S. The figures are rather meager, but as far as they go are strongly suggestive. As the experiments were designed only as preparatory to the investigation proper, to point to possible results and expose the difficulties in the way, the results were counted sufficient, and preparations were made for more careful experiments along the same line.

The apparatus used in the later work differed materially from that which was described above in the account of the pre-

liminary investigation. It was desired that there should be some means of measuring the relative intensities of the shadows, and as this was practically impossible when they were cast by light transmitted through the screen, it was decided to try the effect of casting the shadows directly upon the front of the screen. This of course necessitated the removal of the angles used in casting the shadows to some distance from the screen, in order that they might not be in the subject's line of vision, and this in turn demanded the use of light radiating from a very small area, that the shadows might be sharply defined. The best form of illumination available for this purpose was the electric arc, and as it is not possible to maintain the intensities of two arc lights at anything like a constant ratio, it was necessary to cast the shadows from the same source of light which furnished the general illumination of the screen. This was effected by the aid of mirrors, which diverted in opposite directions the light coming from the lamp, and combined it again upon the screen. An automatic adjustment lamp was first tried;

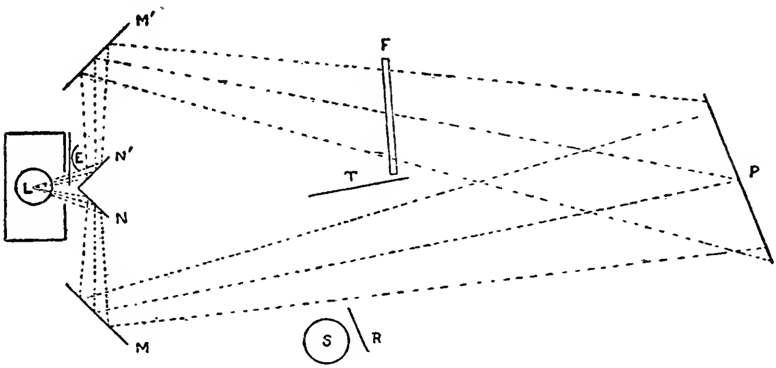

FIG. 3.

but as it proved too unsteady, a hand adjustment lamp, with carbons at right angles to each other, was constructed in the laboratory and found more satisfactory. The arrangement of the lamp with the other parts of the apparatus is shown by the diagram in Fig. 3. The lamp was placed in a box having a blackened interior and suitable apertures for the emission of light from the front, and for ventilation. The light reflected by mirrors *N* and *M* fell upon the screen *P* without interven-

tion, but that reflected by *N'* and *M'* was intercepted by the episkotister *E*, by which any desired proportion of the light was cut out. This light passed through the frame *F*, in which were suspended on silk fibers angles measuring sixty degrees (these being more favorable to the illusion effect than those of ninety degrees used in the preliminary work), and cast their shadows on the screen *P*. This screen was the one used in the preliminary work, with the exception of the angles at the back, which were superfluous in the present arrangement, and were therefore removed. It was placed in a position perpendicular to the line of vision of the subject at *S*, who was seated at one side, out of the path of the light, and hence the distribution of the light on the face of the screen was slightly unsymmetrical. This was unavoidable, however, on account of the dimensions of the room which was selected as best adapted to the purposes of the experiment. The walls, floor and ceiling of the room were black, preventing any great reflection of light back to the screen.

Some difficulty was experienced with the mirrors, and those of carefully selected plate-glass used in the experiment were not thoroughly satisfactory. Better results could probably have been obtained by the use of lenses and totally reflecting prisms.

The fibers by which the angle casting the right-hand shadow was suspended were attached to slides working in the frame *F*, so that the angle could be moved along as the right-hand cross-line was moved on the screen, thus keeping the vertex of the shadow approximately on the cross-line on the screen *P*. The principal remaining pieces of apparatus were screens, one at *R* to prevent the subject seeing the figure at *P* except at the proper time; and one at *T* to conceal the angles in the frame *F* at all times. The distance from the subject to the figure was about two and a quarter meters.

It will be observed that since the two pencils of light were taken from practically the same side of the glowing carbon, the effective area of which was only a few millimeters in extent, variations in the intensity of the light affected them both approximately equally as regards their initial intensities, and hence did not change the relative intensities as established by the episkotister. Since therefore the intensity of the light did

not vary greatly, the perceptibility of the shadows should have remained constant according to the general statement of Weber's law.

What has been said concerning methods in the preliminary work will, with some important exceptions, apply to the main work also. The proper intensity of the shadows was determined for each subject by careful tests, and these were frequently repeated during the course of the experiment to insure the correctness of the intensity adopted. It was found that subjects A., R. and W. could not detect any shadows when the total angular opening in the episkotister was seven degrees. For the sake of safety six degrees was used in the actual work with these subjects. S. at one time seemed to perceive the shadows with an aperture of seven degrees, and therefore five degrees was fixed upon as entirely safe for him. These proportions are rather large as compared with the figures usually given for the difference-threshold of light, but it should be remembered that the shadows were not perfectly distinct, on account of the effect of the fringe of luminosity surrounding the effective portion of the positive carbon, and also on account of multiple reflection in the mirrors.

As it took some time to change the direction of the angles in the frame, they were placed at the beginning of each experiment-period in the direction determined by lot, and continued in the same direction during the whole period (three-quarters of an hour), unless (as happened some few times) the subject accidentally became aware of the direction of the angles. In such a case a coin was tossed to determine whether or not the angles should be reversed, the subject being ignorant of the outcome of the lot, and so not knowing whether the shadows were continued in the position in which he saw them, or were reversed, or were entirely removed and a shadowless series commenced. Towards the end of the work the angles were arbitrarily placed so as to have an equal number of series for each of the two directions of the illusion. To economize time, the angle which cast the right shadow was not moved each time the corresponding cross-line was moved, but only every fifth time, the width of the shadow allowing that amount of movement without complete disconnection.

No attempt was made to run series in both directions, as it was not indispensable for the comparison of the series, and there were enough complications without this additional factor. All the series were run outward from a point nearer to the center than the standard length of twenty-five centimeters.

When the shadows were removed, in the shadowless series, by cutting off the light normally reflected by mirrors M' and N', the episkotister was kept running, and the angle was moved in the frame at the usual times in these series as in the others, so that the subject had no clue as to whether or not the shadows were present in any series.

For fear lest the imperfections in the mirrors might produce irregular distribution of the light on the screen in such a way as to affect the results of the series, the two mirrors on one side were interchanged from time to time with those on the other side, and an equal number of series taken in each of the two positions. There were thus three conditions to be equalized, viz: (1) Position of the mirrors; (2) Direction of the illusion; (3) Order of series in the set, *i. e.*, whether the series with the shadows was given first, immediately succeeded by the series without the shadows, or *vice versa;* in the scheme which follows the first class are called 'shadows first' and the second 'shadows last.'

This classification broke the sets of series up into eight groups, which were kept equal in the long run (with one exception, to be mentioned later), although the sets into which the series fell were determined by lot from day to day as far as possible. These groups are given schematically in the following table:

1st position of mirrors.	Illusion long.	Shadows first.
		Shadows last.
	Illusion short.	Shadows first.
		Shadows last.
2d position of mirrors.	Illusion long.	Shadows first.
		Shadows last.
	Illusion short.	Shadows first.
		Shadows last.

On account of the complication of the apparatus and the difficulties of its operation, the progress of the experiment was necessarily slow. No attempt was made to have the subject judge at regularly timed intervals. An endeavor was made, however, to give all series with the same approximate rapidity, and the rate of ten minutes to a set was pretty constantly maintained. This allowed the taking of three sets or pairs in a period of forty-five minutes, if all worked smoothly, the rest of the time being consumed in the necessary adjustments of the apparatus. Very frequently, however, troublesome delays occurred, which reduced the number of sets to two or one.

Each subject was instructed to preserve a fixed method of bringing his eyes upon the line. One preferred to bring his eyes upward from the floor to the middle of the line after removing the swinging screen, while the others preferred to gaze at the swinging screen, in the direction of the center of the line, and then displace it. This regularity was insisted upon because it was found by actual trial that the direction in which the eyes were brought upon the screen influenced the proportional estimate of the segments of the line. Therefore a difference in this respect between the different series might introduce a difference in the results which would confuse the interpretation.

The subject was not however compelled to maintain his gaze fixed upon the central point, but moved his eyes over the line as he pleased in forming his judgment. In the extreme cases the disparity of the segments of the line was perceived by glancing at the center, but where the difference was very slight the subject gazed at one segment until he had its length well in mind and then transferred his regard to the other segment in order to make comparison.

It is probable, however, that the steady fixation of the segments was on the whole a bad practice, as it rendered possible the formation of an after-image of the first segment by which the second segment might be judged by mere superposition, which would be in a large measure destructive of the effect of the illusion, if there were any such effect. The difficulties of judging were however so great that it was not deemed advisable to place any additional restrictions on the subject.

The experiment would naturally have been much simplified if the shadowless series had been omitted, and the sets of two made to consist of one with the illusion short and one with it long. This would have reduced the number of groups to four, and would perhaps have nearly doubled the difference between the series on each set, but it was deemed inadvisable for several reasons. First, the changing of the direction of the angles for each series would have consumed so much time that the gain by omitting the shadowless series would have been largely offset. Second, the data for the judgments of the segments of the line with the influence of the illusion excluded are important in themselves. And third, it was considered possible that under the conditions, the illusion, if effective at all, might be more effective in one position or direction than in the other; a state of affairs of which the proposed method would offer no evidence, should it actually exist. The results as set forth later justify the procedure actually adopted, although additional experiments ought to be made in the proposed manner to supplement the results of these.

One more point as to the methods, and then we may pass on to the next division of the subject. The series were always commenced far enough inside of the actual equality-point to give a distinct impression of shortness to the right segment of the line. They were not however commenced uniformly at the same point, but were varied irregularly in this respect from set to set, and the subject was given to understand that they were irregular, lest the distribution of his judgments in one series should influence him in the next. Both of the two series of each set were of course commenced at the same point, so that any possible effect of the length of the series, or the strength of the contrast with which the series began, might affect them both alike; but this was not mentioned to the subject, who consequently never knew whether a given series began at the same point as the preceding. It is interesting to note that the general region of commencing the series was necessarily varied from day to day and even from set to set on account of the varying relative estimation of the segments of the line. Thus a point which one day was well within the range of judgments

of 'short' might be the next day within the range of judgments of 'equal.' This will be further exemplified later in the discussion.

Four subjects were employed, three of whom, S., R. and A., had taken part in the preliminary work. In the cases of S. and R. eighty series (forty sets) were taken from each, evenly distributed according to the scheme of the eight classes of sets already specified; but in the case of A. only sixty-four series (thirty-two sets) evenly distributed, and in the case of the other subject, W., seventy-two series (thirty-six sets, twenty under the class 'long' and sixteen under the class 'short') were obtained.

The most obvious method of comparison of the series was by sets, taken in the same manner as that in which the series in the preliminary work were compared. The same precautions for impartiality in computation were observed here also. The results of this comparison as given in Table II. are far more striking than the results for the preliminary work.

Table II.

Subject.	Total Sets.	For.	Against.	Neutral.
S.	40	23	15	2
W.	36	19	11	6
R.	40	25	9	6
A.	32	22	7	3

Expressed in percentages this table becomes Table III., as follows:

Table III.

Subject.	% for.	% against.	% neutral.
S.	57.5	37.5	5.0
W.	52.8	30.5	16.7
R.	62.5	22.5	15.0
A.	68.8	21.9	9.3

This shows a preponderance for the illusion; twenty per cent. for S., over thirty-two per cent. for W., forty per cent. for R. and over forty-six per cent. for A. Or, if we leave out of account the neutral cases and give directly the ratio of favorable to unfavorable cases, we find the following values: S., 1.5.

W., 1.7; R., 2.8, and A., 3.1, which are certainly too large and agree too well to be set down as the result of mere chance.

A second comparison naturally suggested would be between the average of the mean equality-points for the shadowless series and the averages for the series with the illusion-figure in both positions. An examination of the series, however, showed that the average for all of the shadowless series could not be fairly compared with the averages for each of the other two classes, since the series for the two latter occurred in groups irregularly distributed throughout the time the experiment was continued, and the general position of the mean equality-point varied greatly during the progress of the work.

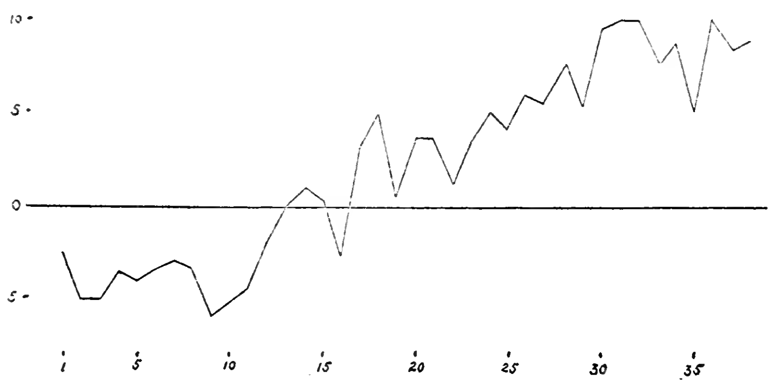

FIG. 4.

This is illustrated by Fig. 4, which gives the position of the mean apparent equality-points for each of the shadowless series of experiments with subject W. In this diagram the values of the abscissa represent the order of the series in the course of the experiment, while the ordinates give the difference in millimeters between the standard line and that length of the variable which seemed equal to it. Positive ordinates, therefore, show that the apparently equal variable was actually longer than the standard, while negative ordinates indicate that it was shorter.

All of the graphs thus obtained were very irregular, and those for subjects W., R. and A. show a decided upward tendency from first to last, indicating that while early in the experiment the right segment was proportionately overestimated, later it

was underestimated. Subject S., with a single exception, overestimated the right segment throughout the experiment, rather more toward the last than the first.

The graphs must not be understood as analogous to the filling in of the points of a probable curve between known points, as in this case the points represent discrete series and there are no possible points which might be computed between them. Only the known points are significant therefore, the lines joining them having been added to distinguish the trend of the series and make it more easily comprehended by the eye.

The ordinates for the series with shadows would correspond pretty closely with the ordinates for the shadowless series with which they belong; but, as previously noted, the two classes of series with shadows were not distributed regularly over the interval covered by the shadowless series. In the case of one of the subjects, for example, the fall of the lots was such that most of the series with illusion 'long' occurred at the upper position of the graph for the shadowless series, and those with illusion 'short' at the lower positions.

It would, therefore, be manifestly incorrect to compare series corresponding to the upper part of the graph with series corresponding to both upper and lower parts, as this would bring in a difference due merely to the progress of the experiment.

Table IV.

AVERAGE MEAN EQUALITY POINTS.

Subject.	Illusion Long.		Diff.	Illusion Short.		Diff.
	Shadowless.	Shadow.		Shadowless.	Shadow.	
S.	—6.42	—5.8	+.62	—5.57	—5.75	— .18
W.	+2.37	+2.77	+.40	+2.65	+1.75	— .10
R.	—3.57	—3.40	+.17	—2.40	—3.45	—1.05
A.	—0.37	+0.46	+.83	+1.06	+0.78	— .72

Hence in computing the averages, those for the shadowless series which were taken at the same time as the series with the illusion 'long' were kept separate from the averages for the shadowless series taken at the same time as the series in which the illusion was 'short.' In other words, the averages for the two classes of pairs, as distinguished by the direction of the

illusion, were taken independently. The results for this operation are given in Table IV.

The figures in the first, second, fourth and fifth columns of this table represent the distance in millimeters from the actual

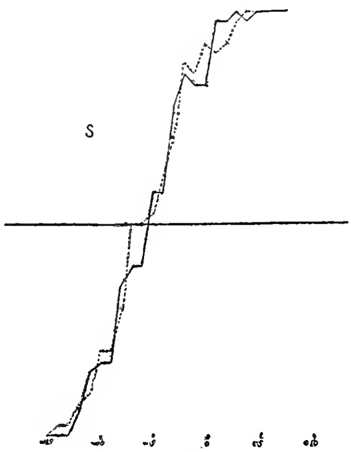

FIG. 5.

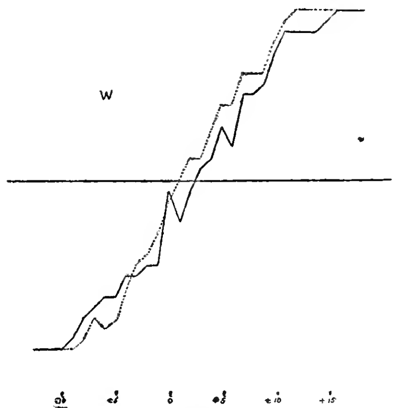

FIG. 6.

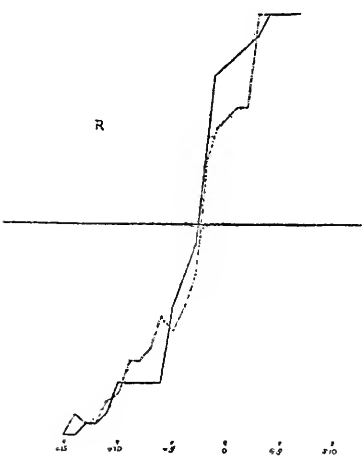

FIG. 7.

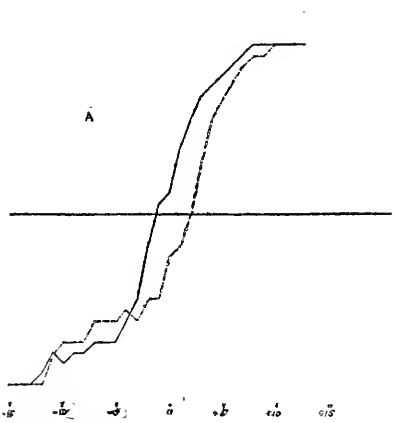

FIG. 8.

equality point to the average mean equality point for each group of series as indicated by the column headings. Negative distances are measured from the actual equality point towards the center of the line in the illusion-figure, and positive distances in

the opposite direction. The figures in the third and sixth columns give the differences of the averages, and hence directly the average change in length of the right segment when the imperceptible shadows were present.

It will be seen from Table IV. that in every case the average mean equality point for the series with shadows is farther out than that for the shadowless series when the illusion is 'long,' and just the reverse when the illusion is 'short'; which is exactly what might be expected if the illusion were really operative. This unanimity is certainly strongly in favor of the theory that the imperceptible shadows actually affect the judgment. The difference here, as in the other comparisons, is slight, but we should hardly expect to get more than a slight effect from the shadows under the circumstances.

This method of comparison by means of equality points necessarily leaves out of account certain scattering judgments which are bound to occur from time to time, and which are ignored by the fixed rules under which the mean equality point was determined in each series. We must, therefore, have some other method of comparison which shall take these into account, although their importance is not so great as is that of the more regularly occurring judgments. The sporadic judgment, contradicted by those which immediately precede and follow it, is of course largely due to a sudden subjective change in the observer, but its significance lies in the fact that the objective conditions might allow these subjective conditions to be more effective in one case than in another.

The method adopted for doing justice to these scattering judgments was the comparison of the total number of judgments of each kind (*i. e.*, 'longer,' 'shorter,' and 'equal' or 'doubtful') for each value of the variable lines on the screen, for each of the two classes of series with the illusion shadows, with the corresponding classes without the shadows. Thus, for example, the totals of the different kinds of judgments when the variable was 23.5 cm. (the standard being 25 cm.), when the illusion was 'long,' were compared with the totals for the same point in the corresponding shadowless series; and so for all of the other lengths of the variable which were used in the experiment.

Here as before, the precaution was taken to compare each of the two classes of series in which the shadows were present only with the shadowless series taken in the same sets.

There being three kinds of judgments, viz., 'longer,'

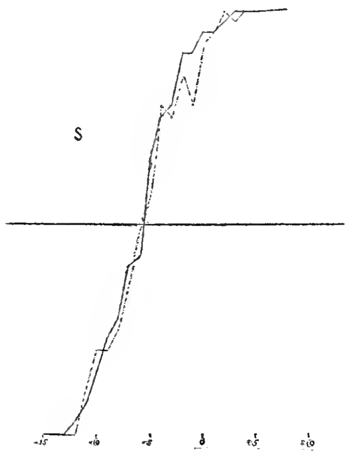

FIG. 9.

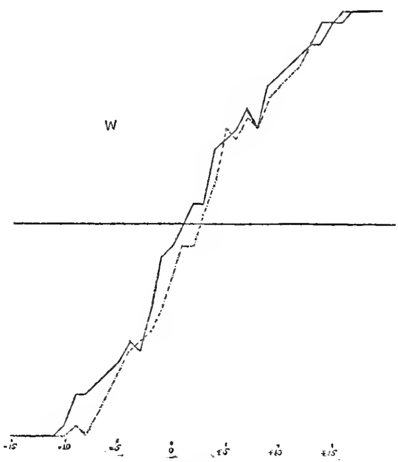

FIG. 10.

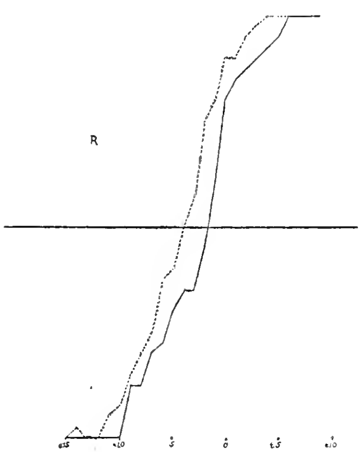

FIG. 11.

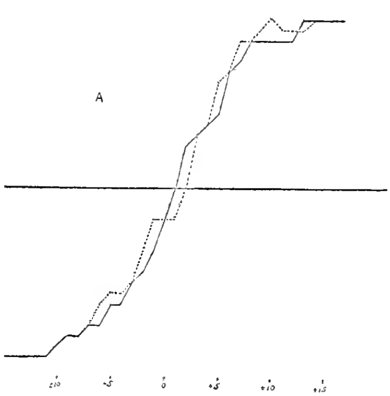

FIG. 12.

'shorter' and 'equal' or 'doubtful,' there were then for each class three totals for each point in the scale. As each of these totals is significant only in so far as account is taken of the distribution of the remaining judgments between the two remain-

ing totals, no direct comparison of the total judgments 'longer,' *e. g.*, in the shadowless series, with similar judgments in the opposite series could be made with advantage. Accordingly the judgments of 'short' were taken as negative, the judgments of 'long' as positive, and the judgments of 'equal' or 'doubtful' as zero. The algebraic sum of these judgments then, at a single point on the scale when the shadows were present, or, on the other hand, when they were absent, fairly represents the character of the total of the judgments at that point. As the series were always begun well within the region of apparent 'shortness' and ended well outside of the region of equality, it was assumed that it was possible to supply judgments at either end of those series that did not reach the limits of the extent of the scale taken for comparison, thus keeping equal the total number of judgments throughout the scale.

Figs. 5 to 12 give in a graphic form the results of this comparison. The abscissas represent the different lengths of the variable line expressed as distances in millimeters from the point where standard and variable were equal; negative values, of course, representing a variable shorter than the standard, and positive a longer. Hence the values here given, plus twenty-five centimeters, show the actual length of the variable. The ordinates represent the algebraic sum of positive, negative and zero judgments. The continuous line gives the value for shadowless series, the dotted line that for series with shadows—in Figs. 5 to 8 so placed as to give the illusion 'short'; in Figs. 9 to 12 'long.'

When treated in this way the results are less striking than in the case of the tables of general averages. If an effect of imperceptible shadows were to appear in the graphs, it would in Figs. 5 to 8 be evidenced by the fact that the ordinates of points on the dotted lines would incline more toward positive values than the corresponding ordinates of the continuous lines; and more toward negative values in Figs. 8 to 12. This would bring the dotted line to the left of the continuous line in the first group of figures, and to the right in the second group. In some of the actual curves, however, the dotted line shows no decided tendency either way (Figs. 5, 8 and 9). In Figs. 6, 10 and

11 the relation is faintly in accord with what we should expect if the shadows were effective; while in 7 and 12 the agreement is quite striking. If this accord were a mere matter of chance, several of the curves ought to be not simply neutral, but positively opposed to this course. None of the graphs, however, has this character. So that, on the whole, this third mode of treating the results also is favorable to the view that the shadows were operative in determining the judgment.

We may now sum up the results of our investigation. We have found three main points. First, when we compare the series set by set as they were taken, we find that for each subject the large majority of the sets compare as they should if the illusion were operative. Second, we find that the averages of the sets compare without exception as they should under the foregoing hypothesis, a result which does not necessarily follow from the foregoing result. Third, we find that the totals of the judgments at the various points on the scale are, to a remarkable extent, in conformity with the same hypothesis. These three methods of comparison cover every relation of the series which can fairly demand attention, and hence may be taken as exhaustive of the results of the present experiment.

A question of such importance as that with which the present discussion deals requires, however, the maximum of careful investigation before we dare call it settled. The results detailed above strongly suggest that the imperceptible illusion-figure is active in producing psychical results, but for the sake of conclusiveness additional experiments should be carried out so as to permit the comparison of series in which the shadows are in one position directly with other series in which the shadows are in the opposite position, using a modification of the method of right and wrong cases, consisting in plotting the error curve for the progressive intensities of the shadows by the method of Least Squares, and then comparing this with the empirical results obtained by the actual trial of the different intensities. This method of procedure is not absolutely necessary, but is advisable on account of the added satisfaction derived from approaching the same problem by diverse methods.

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STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF CALIFORNIA.

COMMUNICATED BY PROFESSOR GEORGE M. STRATTON.

III. Visible Motion and the Space Threshold.

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IV The Method of Serial Groups —

V. The Effect of Subdivisions on the Visual Estimate of Time.

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III. Visible Motion and the Space Threshold.

BY PROFESSOR GEORGE M. STRATTON.

For some time it has been argued that the perception of motion has no immediate connection with the discrimination of positions in space. It is held that the two processes are psychologically independent, and that we become aware of motion by a direct and simple sense of the motion itself, and not by appreciating that the object occupies distinct localities.

This paradox is supported in part by the fact that a movement in the out-lying portions of the visual field can be readily seen when the movement has a far less extent than is required to give to two motionless objects a perceptible difference of position,—a fact apparently first observed by Exner. To state the matter in the words of Professor James: "One's fingers when cast upon the peripheral portions of the retina cannot be counted—that is to say, the five retinal tracts which they occupy are not distinctly apprehended by the mind as five separate positions in space—and yet the slightest *movement* of the fingers is most vividly perceived as movement and nothing else. It is thus certain that our sense of movement, being so much more delicate than our sense of position, cannot possibly be derived from it. * * * Movement is a primitive form of sensibility."[1]

[1] 'Principles of Psychology,' II., 172.

One may rightly have some antecedent hesitancy, I believe, in regard to James' interpretation. Even admitting the facts he offers, one need by no means draw his conclusion. For the alleged sense of motion, if it really is a sense of *motion*, brings in a spatial report. The changes of which it makes us aware are preceptibly different from alterations merely of intensity or of color. Even though we may be unable to tell the direction of the motion, the motion itself is a change of position, and is dimly appreciated as such. Instead of saying, then, that the experiments cited are evidence that the sense of movement is so much more delicate than the sense of position, it would be more exact to say that they show that the discrimination of positions during movement is much finer than the discrimination of positions at rest. It is not really an antithesis between 'motion' and 'differences of position,' but between differences of position under two contrasting sets of conditions, motion being but a special mode of testing our power of local discrimination. The truth seems to be that there are various ways of measuring this power—among others, by the simultaneous presentation of two lights in different places, by their successive presentation, or by a continuous movement of a single light from one position to another. We have no right to assume (as Professor James seems to do) that the first of these methods is the only one that gives the true space-threshold, and that the results of the third, if finer, are indications of a process different in kind. The second method also gives finer results than the first, and yet no one, so far as I know, has thought that the finer space results obtained by successive stimulation implied some special and primitive form of sensibility different from that which is involved in discriminating simultaneous impressions. Why, then, should we jump to this conclusion when the conditions of space-perception are only slightly altered farther, making the successive stimulation spatially continuous instead of discrete?

Admitting the facts adduced, then it by no means follows that we have a primitive sense of movement, independent of spatial discrimination. But at least so far as space is concerned, the facts themselves are not unquestionable. Some experiments

by Stern, reported in 1894,[1] already raised a doubt here. Stern found that when retinal irradiation was decidedly reduced, the shortest perceptible movement was not appreciably less than the space-threshold as determined without movement. He consequently inferred that there was no ground for assuming a specific and unique sense of movement. But the test of motionless space-discrimination used by Stern is itself perhaps not fully convincing. He departed from the method employed by Helmholtz and others — the method by parallel lines brought closer and closer together until they almost fused — and, instead, took as the threshold that width of a single dark line that was just doubtfully perceptible against a light background. The assumption here seems to be that the line is a gap or interruption of the white surface, and implies a local discrimination of the two borders of white against the line which divides them. But if we regard black as a positive impression, as it would seem we must, there would appear to be no reason why a consistent development of this method would not require us to accept the apparent diameter of a just perceptible fixed star as a still more accurate measure of local discrimination. The width of the dark line in Stern's experiments is probably of importance only in a secondary way, by affecting the intensity of the impression of black. This suspicion is strengthened by the fact that with some slight improvements in the conditions of observation in our laboratory, while still remaining true to the general principle of Stern's method, the threshold takes an astonishing drop. One of my students, Mr. Gilbertson, finds that the width of the black line visible against white, instead of being 15″ angular measure, as Stern found under his conditions, may be even less than 2.5″.

For this reason it seemed well to try some experiments in which the threshold of motion and that of local discrimination might be compared without so much doubt as to the really spatial character of this local discrimination. The experiments here reported fall into two groups, the first with indirect, the second with foveal vision.

[1] Die Wahrnehmung von Bewegungen vermittelst des Auges, *Zeitsch. f. Psychol. u. Physiol. d. Sinnesorg.*, VII., 321.

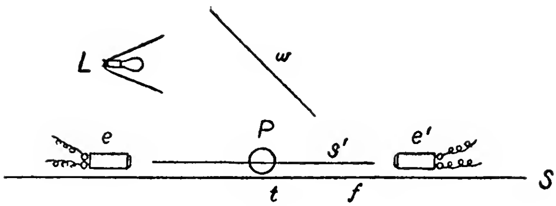

FIG. 1.

I. *Experiments with Indirect Vision.*—The observer, in Fig. 1, sat in a half-darkened room and judged the character of the light-stimulus seen through a narrow vertical slit *t* about ½ mm. wide in a screen *S* before him. The fixation point *f* was 5° to the right of the slit in one set of experiments and 30° to the right in the other, the distance from the observer to the screen being in these two cases 2 m. and 1 m., respectively. When the aim was to determine the threshold of motion, a bright point of light moved from a fixed position upward or downward in the slit, while the local discrimination was tested by two separate motionless points of light which appeared the one above the other and in immediate succession. The extent of movement, in the one case, and of spatial separation in the other, were of course accurately varied to determine the threshold.

This variation, along with a constancy of those conditions that should remain constant, was brought about by an arrangement behind the screen, consisting chiefly of a pendulum *P* carrying a smaller slitted screen *S'*, which in swinging past the slit *t* allowed light to pass to the observer from a white surface *w* evenly illuminated by an electric lamp, *L*. The pendulum was so controlled by the electromagnets *e* and *e'* as to give a single swing from a fixed point 10° from the vertical and be caught at the end of its course on the other side. The back-swing in preparation for the next experiment was concealed from the observer. The length of the pendulum was regulated to give a single oscillation in one second.

In Fig. 2 there is represented in diagram the screen *S'* that was carried by the pendulum when the aim was to give the observer a moving point of light. The main feature of this

screen was a movable circular disc D, from whose center, c, there passed to the periphery a slit cp, ½ mm. in width, forming an arc of the circle whose radius was the distance from C to the point of suspension of the pendulum (70 cm.). When the

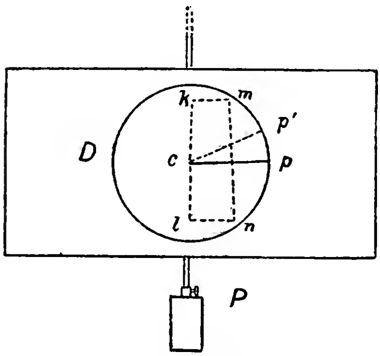

FIG. 2.

disc was so set that the center of this arc coincided with the point of suspension of the pendulum, the point of light which came to the observer as this arc-slit swung past the vertical slit t in the stationary screen S in Fig. 1 had no apparent motion whatever. A change of the disc's position, however, so that the slit took the position cp', would cause an apparent upward motion of the point of light, the extent of which in angular measure, from the point of view of the observer, could readily be calculated. A scale in terms of such angular measurement was placed along the circumference of the disc so that this could be set for any extent of motion desired. But since it seemed best to keep the duration of the movement exactly the same as the duration of the stimulus in the corresponding set of experiments with motionless points, this constancy, in spite of the varying positions of the disc, was maintained by the fact that the screen S' was continuous behind the disc D with the exception of an opening $klmn$, whose boundaries kl and mn were radii of the circle mentioned above, the center of which was the point of suspension of the pendulum. It need only be added that this screen was wide enough to conceal the surface w in Fig. 1, when the pendulum was at either limit of its excursion.

For the experiments on the discrimination of position without movement, a different screen was substituted upon the pendulum, yet with a similar arc-slit similarly centered. But in this case, instead of revolving the slit about a central point, one half of the slit could be shifted slightly up or down, along the radius of the circle of which the arc was a portion, as in

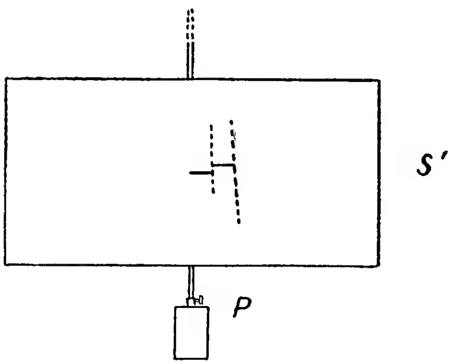

Fig. 3.

Fig. 3. As the pendulum made its single swing, there thus appeared to the observer in immediate succession two separate points of light, the amount of whose separation could be accurately varied by the experimenter.

In the preliminary experiments, the well-known method of 'minimal changes' was employed, but the observers soon showed themselves so much influenced by expectation that no reliable determination of the thresholds seemed possible by this means. The method of 'serial groups,' hereinafter described, was found more satisfactory. The adoption of successive stimuli to measure the local discrimination without movement, was for the purpose of avoiding some of the worst difficulties from irradiation. Two points of light when presented simultaneously will often produce a blur that is perceptibly extended (and consequently spatial), and yet two separate nuclei of light are not distinguished within this vague extent. According to the usual method of interpreting the results, we should here be below the space-threshold, although, of course, we are not. What Tawney has already justly insisted on with regard to

tactual impressions,[1] that the perception of two points is not a true measure of the threshold, might consequently be repeated with respect to vision. The fusion of the two points is largely obviated by the use of successive stimuli so that the intensity of the first impression is perceptibly lessened by the time the second is at its height.

But the substitution of successive for simultaneous impressions brings what some might consider a vitiation of the results. The two points of light, even when well separated, give the psychological impression of continuous motion; the light seems to leap from the position of the first point to that of the second. At the beginning it was thought possible to separate those judgments in which an apparent motion entered, and treat them as a questionable group by themselves. But when once the suggestion of motion becomes fairly lodged in the observer's mind, it occurs so persistently and over such a range that this attempt was renounced. Since there is no objective movement here, however, and the whole thing is really a matter of suggestion pure and simple, and seems to me to imply an underlying space-discrimination as the basis of the suggestion itself (as I have urged at the beginning of this paper), the judgments with subjective motion have been freely employed in computing the thresholds. Those to whom these reasons do not seem sufficient may have no such scruples over the second group of experiments on foveal vision; not even suggested movement there enters into the judgment.

The results of the experiments with indirect vision were as follows:

Observer.	Angle of Observation.	Number of Determinations.	Discrim. of Position.		Perception of Motion.	
			Threshold.	M. V.	Threshold.	M. V.
A	5°	6	9.2′	1.6′	10.4′	1.4′
	30°	5	29.0′	5.2′	29.0′	3.2′
Bd	5°	6	7.9′	.7′	8.3′	1.9′
	30°	3	18.3′	2.2′	21.7′	5.6′
Bl	5°	3	7.5′	3.3′	6.6′	2.2′
D	30°	4	63.7′	8.7′	70.0′	10.0′

[1] 'The Perception of Two Points Not the Space Threshold,' PSYCHOL. REVIEW, II., 585.

The table indicates that where the conditions are practically equal, the perception of movement has no advantage over the discrimination of position without objective movement. So far as indirect vision is concerned, the theory that the two processes are psychologically independent here finds no support.

II. *Experiments with Direct Vision.*—The second group of experiments dealt with the same problem as the preceding, with a change merely in the method of investigation and with a different portion of the retina—the foveal instead of the excentric region of sight. On account of the extraordinary nicety of our space discrimination under these conditions, the observer was placed in a distant building where there was an unobstructed view of one of the laboratory windows, the distance of his station from the object to be observed amounting by accurate survey to 120 m. The observer faced the north, and the portion of the laboratory towards which he looked sent back no direct glare from the sun; preliminary experiments showed the need of regarding these things. The distance of the observer's station, it is true, was inconvenient in many ways, but this was more than offset by the greater ease of observation. The naked eye could now be employed, and all the difficulty was avoided that comes from the use of a reducing lens, such as Stern found necessary. The fact that the threshold of movement easily ran down to about one half of what he reports shows the advantage of these conditions. For the perception of motion a strip of white bristol-board, 8 mm. wide by 50 cm. long, was mounted vertically on a dead-black frame which could be moved horizontally before a larger dead-black background. From the observer's station the frame itself was invisible, aud all that one saw was the white line on the large black field. The movement of the line was controlled by runners and guides and adjustable stops on the frame, so that the motion was kept horizontal and its excursion varied by steps of 1 mm. The experimenter moved the frame by hand, and during any single observation kept the extent of the movement constant, and continued the oscillations from the moment just before the exposure of the line until the observer had signalled his judgment. In order to insure full justice to this side of the investigation, the *rate* of movement

had, also, of course, to be taken into account; otherwise some velocity might be selected that would not be the most favorable to perception. For this reason the experiments on motion were subdivided into five groups, with a rhythm, respectively, of 60, 100, 180, 300, and 450 single swings to the minute. The rates were maintained with reasonable accuracy by the aid of an adjustable pendulum invisible to the subject of the experiment.

For the corresponding set of experiments on the discrimination of position without movement the conditions were in all respects the same, except that for the moving frame with its single vertical line two vertical lines end-to-end against the dark background were substituted. Of these two lines the lower was fixed, while the upper was capable of being shifted from continuity with the lower line to any position to the right or left, remaining throughout parallel, however, to its original position. The actual settings varied by steps of 1 mm., and the observer had simply to judge whether the two lines at any given setting were continuous, or in what respect they spatially differed.[1]

The results of the two sets of experiments under these conditions were as follows:

Discrimination of Position Without Movement.
Thresholds in mm.

Observer S.		Observer Y.	
Left.	Right.	Left.	Right.
4	4	6	3
7	5	4	6
3	5	5	6
7	5	6	6
3	3	7	4
4	6		
Av. 4.3	4.3	5.6	5.0

The thresholds for the discrimination of position without movement are slightly higher than those obtained in my previous study already referred to, but the difference is not such as to call for any special comment. The thresholds for the perception of movement, as affected by the rate of move-

[1] For a more detailed account of this method, see my 'New Determination of the Minimum Visibile and its Bearing on Localization and Binocular Depth,' Psychol. Review, VII., 429.

Perception of Movement.
Thresholds in mm.

Single Oscillations per Minute.	60	100	180	300	450
Observer S.	11	6	4	4	4
	16	4	4	4	5
	14	7	4	4	4
	8	5	4	4	6
Av.	12	5.5	4	4	4.75
Observer Y.	25	14	7	4	7
	15	14	5	6	7
	18	8	6	6	6
	16	12	6	5	6
Av.	18.5	12	6	5.2	6.5

ment, take a somewhat different course from those reported by Stern.[1] This investigator worked with rhythms of 144, 84 and 72 vibrations to the second, and found *merkwürdigerweise* (as he says) that the slowest movement was the one most distinctly perceived. In my own results, it will be seen that the lower rates, those of 60 and 100, are markedly unfavorable to perception, with a decided improvement as we pass to 180 and 300, the threshold again rising slightly as the rate is still farther increased. Virtue here seems again to lie in the mean. The lower rates require a considerable excursion before they can be distinguished from rest, while the most rapid oscillations tend to produce a mere indistinctness rather than a perceptible swaying of the object. I am quite at a loss to account for our divergent results here, unless it be that by the word '*Schwingungen*,' which Professor Stern uses without modification, he means '*double* vibrations.' In this event his lowest rate would be about midway between my 100 and 180, and he would have caught only the dip of the threshold as it comes out of the region where the rapidity of the impressions produces blur, without his rates becoming slow enough to show the upward trend of the threshold again farther on. My own subjects, like his, never knew beforehand the rate that was being used, so the discrepancy cannot be explained in this way. Possibly the difference is in some way connected with the fact that his observers had to look through the lens of a microscope.

[1] *Zeitschrift für Psychologie u. Physiol. d. Sinnesorg.*, VII., 347.

For observer *S*, the most favorable rates of motion give a threshold of 4 mm. (about 6.8″ angular measure) as against 4.3 mm. (about 7.3″) for the discrimination of place — a net advantage, for motion, of .3 mm. or .5″ of arc, on the average. If, however, we were to disregard averages and look at the individual determinations, it would appear that the space discrimination occurred as low as 3 mm. (5.1″ of arc); while in no case does motion become perceptible below 4 mm. For observer *Y*, the mean threshold for position without movement is 5.3 mm. (9″); while the most favorable rate of motion gives 5.2 mm. (8.8″) on the average. Here again the minimum thresholds for position are below the smallest for movement. Thus, from one point of view, motion has a slight advantage over discrimination without movement, which advantage, however, is reversed when the numbers are differently regarded. But in either case the difference is too slight to serve as a sufficient basis for entirely differentiating the psychological processes involved.

The results here, then, are in substantial agreement with those obtained in indirect vision by an independent method. The doctrine that visual motion is a primitive form of sensibility independent of local discrimination finds no experimental warrant. The perception of motion seems to be nothing more nor less than the perception that a sensation is changing its space relations, the motion itself furnishing a decidedly favorable, but by no means unique, set of conditions for appreciating such differences of space relationship. This does not imply that the detection of movement always involves a deliberate comparison of positions; for the discrimination often undoubtedly occurs at a single psychic stroke. But even this apparently simple stroke is really a complex act. It implies a relational activity of the mind which interprets and gives character (crude and confused though it be) to the incoming sensations, so that they are no longer blank impressions, but are impressions which mean for us *movement*. The experiments thus go to support the view that a fact of space can never be conveyed to the mind in the form of a pure sensation divested of all relationship.

IV. The Method of Serial Groups.

BY PROFESSOR GEORGE M. STRATTON.

In the practical conduct of the laboratory one frequently feels the shortcomings of the method of 'minimal changes.' It is undoubtedly the best all-around mode of procedure yet devised, but in certain cases where the conditions are exceptional it may leave one quite *im Stich*. This is especially true when one is dealing with minimal impressions, where suggestion is apt to find such free play; the observer may continue to notice a sensation when the stimulus has become suspiciously weak—in fact when no stimulus at all is applied. Thus, with certain excellent though suggestible subjects, I have found it impossible to determine by the method of minimal changes, pure and simple, the least extent of visible motion that could be perceived as motion. The subjects persisted in seeing the light move on every occasion, whether there was any actual movement or not. The control of the answers, by requiring that the observer shall tell correctly some additional feature of the impression—tell, say, the direction of the movement, or, if the experiments be on the least perceptible change of pressure, tell whether the pressure becomes heavier or lighter—may in some instances be helpful. But often this check will hide the very facts that one wishes to ascertain—the point at which the subject perceives motion and yet is uncertain of the direction, or notice change of pressure without being able to say whether the weight has grown greater or less.

The usual resort in this event is either to a so-called 'catch experiment' the *Vexirversuch*, where no stimulus at all is given, or to the method of right and wrong cases. The latter, making use as it does of the law of probability, not only requires an extremely large number of observations, but there is usually needed considerable preliminary and irregular experimentation in order to discover the conditions that will give a suitable proportion of right and wrong answers. The *Vexirversuch*, on the other hand, has never been systematized, and as it is usually introduced in an irregular fashion within the

method of minimal changes, it is apt to disturb the even tenor of the research, and disconcert the observer whenever he gets a hint of what is being done.

In the method of serial groups here proposed, the attempt is made to legitimate the 'catch' experiment, to introduce it as a continuous and regular element of the procedure, while securing certain advantages both of the method of minimal changes and of the method of right and wrong cases. To give a concrete illustration, suppose the following groups of experiments be carried out to determine the just perceptible extent of movement by sight, under the conditions described in the second part of the preceding paper:

Group I.			Group II.		
Exp. No.	Amount of Motion.	Judgment.	Exp. No.	Amount of Motion.	Judgment.
1	0	no motion.	11	0	no motion.
2	7 mm.	motion.	12	0	" "
3	7 mm.	"	13	6 mm.	motion.
4	7 mm.	no motion X.	14	6 mm.	"
5	0	" "	15	0	no motion.
6	0	" "	16	6 mm.	motion.
7	7 mm.	motion.	17	0	no motion.
8	0	no motion.	18	6 mm.	motion.
9	0	" "	19	0	slight motion X.
10	7 mm.	motion.	20	6 mm.	motion.

Group III.			Group IV.		
Exp. No.	Amount of Motion.	Judgment.	Exp. No.	Amount of Motion.	Judgment.
21	5 mm.	no motion X.	31	4 mm.	very slight motion.
22	0	" "	32	0	no motion.
23	5 mm.	motion.	33	0	" "
24	0	no motion.	34	0	very slight motion X.
25	5 mm.	" " X.	35	4 mm.	no motion X.
26	0	" "	36	0	" "
27	5 mm.	motion.	37	4 mm.	motion.
28	0	no motion.	38	4 mm.	"
29	0	" "	39	0	no motion.
30	5 mm.	motion.	40	4 mm.	" " X.

The X shows the errors in any group, and from these the threshold may be determined according to any proportion of correct and incorrect answers that may be chosen. In my own computations that group has been taken as giving the threshold beyond which less than eight out of the ten judgments are right. But a detail like this, as well as the exact number of

experiments that may best form a 'group,' might well be considered as subject to revision in the light of farther experience, and not as an essential part of the method. The essence of the matter is simply that there should be groups of experiments arranged in a regular series, the amount of positive stimulus, as one passes from group to group, being graduated according to the principle of the method of minimal changes; while within the limits of any one group a constant stimulus is irregularly alternated with cases where the stimulus is zero, thus uniting in the single group the basal principle of the method of right and wrong cases and that of the *Vexirversuch*. This may seem provokingly eclectic, but it is not exactly that; the different elements make an organic union, and not a mere patchwork. There is simply an attempt to make systematic what experimenters have frequently found themselves compelled to do in a casual and uncritical way.

One may perhaps repeat that this method is not proposed as a general substitute for the classic ones in use. It is well, however, to multiply our tools so that the best may be selected for the special work in hand. And this one has been found good for certain purposes, especially where suggestion plays a prominent rôle. The observer may here know from the very beginning the general method of procedure; he may know that zero-cases are to be irregularly alternated with those of positive stimulation, and his expectation is therefore less 'set' and influential. The zero-cases no longer come in as a kind of indignity upon the observer, as if his word were being questioned. The check here, because of its constancy, ceases to excite any feeling. The procedure, moreover, has the virtue of the method of minimal changes, in that the threshold is ascertained empirically, by actually crossing it. And while the principle of right and wrong cases is employed, with the powerful control which that always brings, yet there is no introduction of the intricate calculus of probability and a certain darkness that always shadows its results. It is true that the application of the method of serial groups is in a certain sense cumbersome, as compared with the method of minimal changes, since in a given time fewer determinations of the threshold can be obtained. But

with suitable rests between the 'groups,' there is no need of there being greater fatigue to the observer in the one case than in the other; and while the determinations may be fewer for the time expended, yet in most cases I have found that they more than make up in weight what they lack in number.

V. The Effect of Subdivisions on the Visual Estimate of Time.

BY MABEL LORENA NELSON.

It has been found by Dr. Ernst Meumann and others that the estimate of small time-intervals is influenced by the number of stimuli that fall within the interval. In the space illusion of sight, a single division of the standard will cause it to be underestimated, while more divisions will cause an overestimation; in touch, the effect of subdivisions depends on the absolute length of the standard.[1]

My object, in the following experiments, was to determine the effect of single and multiple divisions of the standard on times of longer duration than those investigated by Dr. Meumann, and to discover if there existed a temporal illusion comparable to the space illusions of sight and touch.

In Dr. Meumann's investigation of time intervals, he compares an 'empty' time — one bounded by two impressions — with times 'filled' with either three, five, six, nine or twelve impressions, inclusive of the terminal stimuli. His results [2] are, that for times from one tenth of a second to about four seconds, when the filled time comes first, the error in estimating is constantly positive — while for longer times the error is negative.

This seems to indicate that the effect of the filling is positive for the short times, and negative for the longer. The error found by Dr. Meumann is, however, not due to the filling alone, but is the result of two factors. It is generally conceded that even when two empty times are compared, there is a similar constant error, positive for short times, negative for longer.

[1] See the paper by Miss Alice Robertson, on "'Geometric-Optical' Illusions in Touch'" to be published subsequently.

[2] 'Beiträge zur Psychologie des Zeitbewusstseins,' *Phil. Studien*, XII., p. 127.

That there is a difference other than this constant error which must be attributed to the filling, Dr. Meumann shows—for, in those cases where the order is reversed, the empty time coming first, the sign of the error is also reversed — but the quantity of the error due to the filling alone he does not show, as these two factors are not quantitatively separated.

The longest period chosen by Dr. Meumann was nine seconds. The following experiments were taken to determine what effect the filling would have on longer periods, durations of several minutes.

The intervals chosen were one half, one, two, four, six and ten minutes; the filling, sensations of light.

Under each interval two sets of estimates were taken. The first, where the standard and compared times were both empty (marked *E–E* in the tables) was taken to determine the constant error due to the mere sequence of the two intervals. In the second set one of the times was always empty and the other filled (*E–F* and *F–E* in the tables). Any difference found between the estimates of the two sets, for a given interval, must be due to the filling.

The results as given in the tables are computed from five estimates under each interval for the empty time, and five for the filled. The average of the estimates is given; the difference between this and the standard interval, expressed as a per cent. of the standard; and the mean variation from the average estimate, expressed as a per cent. of the average. A second basis of comparison is the median of the five estimates and its difference, as a percentage, from the standard interval.

The effect due to the filling for each interval is found by subtracting the constant error, when both standard and compared times are empty, from the error in estimating when one of the times is filled. When the difference due to the filling in Tables I. and II. has a positive sign, it must be taken to mean that the filled time seemed shorter than an empty one of the same length. In Tables III., IV. and V., however, the order is reversed, the filled time being taken as the standard — a positive error here would indicate that the filled time seemed longer than the empty.

The method of taking the experiments was as follows: The subject sat in a darkened room before a screen and saw through an aperture in the screen, 5 mm. wide by 10 mm. in height, flashes of light through a noiseless pendulum behind. A flash of light marked the beginning of an empty time, a second flash its end. During the filled time the subject saw a flash of light every half second. There was in every experiment a pause of two seconds between the closing flash of the standard and the first flash of the compared time. The end of the compared time was marked off by a word from the subject when a time had elapsed which seemed to him equal to the standard.

In the first group of experiments the standard time was always empty. Two subjects were taken, *D* and *R*; the results are found in Tables I. and II. The great difficulty my subjects found, in the long intervals, in keeping their attention on the length of the standard, made it necessary to give them some idea of the interval to be used. Accordingly they were told whether the interval would be short (one half and one minute were called short), moderate (two and four minutes), or long (six and ten minutes), and whether the compared time would be filled or empty. This was, of course, in some respects a disadvantage, as it perhaps affected the lengths of the estimates, but as my object was to compare the estimates of a filled and an empty interval of time, the results are not invalidated by this guidance, as it was given alike in both sets.

In order that there might be no constant effect due to contrast, the order of using the different lengths as standards was not fixed, but was determined by chance. The time occupied by my work was never more than one hour at a time.

TABLE I.

SUBJECT *D*. *E–E*.

Interval.	Av. Estimate.	D %	M.V. %	Median.	D %	M.V. %
½ min.	28.2 secs.	— 6	27.5	34 secs.	+ 13	19.4
1 "	45.4 "	— 24.3	10.7	44 "	— 26.6	10.4
2 "	1 min. 40.6 "	— 16	12.6	1 min. 40 "	— 16.6	12.6
4 "	2 " 58.2 "	— 25.7	30.2	2 " 26 "	— 39.1	33.7
6 "	4 " 56.8 "	— 17	14.7	4 " 43 "	— 26.9	14.4
10 "	7 " 21.4 "	— 26.4	31	7 " 22 "	— 26.6	34.2

E–F.

½ min.	53.4 secs.	+ 78	40.1	43 secs.	+ 43.3	35.3
1 "	1 min. 18.6 "	+ 31	15.4	1 min. 17 "	+ 28.3	23.4
2 "	2 " 6.6 "	+ 5.5	30.4	1 " 55 "	— 4.1	35.1
4 "	4 " 6.8 "	+ 2.8	27.1	4 " 20 "	+ 8.3	27.7
6 "	6 " 7.4 "	+ 2	33.5	6 " 53 "	+ 14.7	27.5
10 "	7 " 1.8 "	— 29.7	18.4	7 " 26 "	— 25.6	16.3

Difference due to Filling.

	Average.	Median.
½ min.	+ 84 per cent.	+ 30 per cent.
1 "	+ 55.3 "	+ 54.9 "
2 "	+ 21.5 "	+ 12.5 "
4 "	+ 28.5 "	+ 47.4 "
6 "	+ 19 "	+ 41.6 "
10 "	— 3.3 "	+ 1 "

The results as given in Table I. show that when the average is taken as the basis of comparison, the effect of the filling on the estimates in the case of *D* is very constant. In the interval of a half minute, a filled time 84 per cent. greater than the empty is taken to be its equal. The effect of the filling seems to decrease as the length of the interval increases, until at ten minutes it is but little or nothing.

Table II.

Subject *R.* *E–E.*

Interval.	Av. Estimate.	D %	M.V. %	Median.	D %	M.V. %
½ min.	34.4 secs.	+ 14.6	26.2	34 secs.	+ 13	25.9
1 "	1 min. 12.2 "	+ 20.6	23.1	1 min. 13 "	+ 21.6	23.5
2 "	1 " 37.8 "	— 18.5	12.8	1 " 39 "	— 17.5	17.4
4 "	3 " 12.8 "	— 19.6	16.3	3 " 17 "	— 17.9	14.5
6 "	3 " 50.2 "	— 36	18.3	3 " 11 "	— 46.9	29.1
10 "	5 " 35.1 "	— 44	18	4 " 55 "	— 50.8	15.1

E–F.

½ min.	36.2 secs.	+ 20.6	33.5	40 secs.	+ 33.3	28.5
1 "	57.4 "	— 4.5	27.3	54 "	— 10	27.4
2 "	1 min. 47.4 "	— 10.5	29.8	1 min. 39 "	— 17.5	30.7
4 "	2 " 56.2 "	— 26.5	36.4	3 " 7 "	— 22	33.1
6 "	3 " 45 "	— 37.5	15.4	3 " 33 "	— 40.8	17.3
10 "	5 " 33 "	— 44.5	8.4	5 " 48 "	— 42	7.5

Difference due to Filling.

	Average.	Median.
½ min.	+ 6 per cent.	+ 20.3 per cent.
1 "	— 25.1 "	— 31.6 "
2 "	+ 8.5 "	0 "
4 "	— 6.9 "	— 4.1 "
6 "	— 1.5 "	+ 6.1 "
10 "	— .5 "	+ 8.8 "

With *R*, Table II., the filled time seems shorter than the empty in the intervals of one half and two minutes, but for all the other times it seems longer, taking the average as the basis of comparison. The error in the longest intervals is very small and its sign is changed when the median is taken instead of the average.

In continuing the experiments I introduced the filling into the first or standard time, the compared time being always empty. We should, therefore, expect a reversal of sign in the effect due to the filling. I found that after the former practice the subjects were able to hold their attention to the standard time without any foreknowledge as to its length. In this second group of experiments they were consequently ignorant of the character of the interval to be used. In all other respects the work was conducted exactly as before.

Table III.

Subject *D*. *E–E*.

Interval.	Av. Estimate.	D %	M.V. %	Median.	D %	M.V. %
½ min.	30.8 secs.	+ 2.6	21.3	28 secs.	— 6.6	21.4
1 "	1 min. 10.2 "	+17	29.6	53 "	—11.6	33.3
2 "	2 " 25 "	+20.8	16.6	2 mins. 10 "	+ 8.3	10
4 "	3 " 31.6 "	—11.4	21.2	2 " 47 "	—34.1	18.9
6 "	4 " 58.9 "	—16.9	17.6	4 " 50 "	—19.4	13.4
10 "	9 " 31.4 "	— 4.7	23.8	10 " 40 "	+ 6.6	22.2
			F–E.			
½ min.	35.1 secs.	+17	15.8	33 secs.	+10	13.6
1 "	55.4 "	— 7.6	29	46 "	—26.6	26.5
2 "	1 min. 26.6 "	—27.8	26.7	1 min. 14 "	—38.3	22.1
4 "	3 " 53 "	— 2.9	18.9	4 " 19 "	+ 7.9	15.9
6 "	4 " 43.6 "	—18.4	20.1	4 " 22 "	—27.2	18.2
10 "	9 " 28.2 "	— 5.3	21.7	10 " 1 "	0	19.5

Difference due to Filling.

	Average.	Median.
½ min.	+ 14.4 per cent.	+ 16.6 per cent.
1 "	— 24.6 "	— 15 "
2 "	— 48.6 "	— 46.6 "
4 "	+ 8.5 "	+ 42 "
6 "	— 1.5 "	— 7.8 "
10 "	— .6 "	— 6.6 "

The results for *D*, Table III., show that with two exceptions (one half and four minutes) the difference due to the filling has

a negative sign, where in Table I. it was positive, indicating that in general the filled time seemed shorter than the empty.

Table IV.

Subject *R*. *E–E*.

Interval.	Av. Estimate.	D %	M.V. %	Median.	D %	M.V. %
½ min.	28.2 secs.	— 6	27.7	23.5 secs.	—21.6	29.3
1 "	1 min. 10.8 "	+18	23.4	1 min. "	0	24.1
2 "	1 " 33.2 "	—25.5	24.3	1 " 28 "	—26.6	24.5
4 "	2 " 20.5 "	—41.5	25	2 " 14 "	—44.1	16.5
6 "	3 " 41.4 "	—38.5	19.2	3 " 56 "	—34.4	16.7
10 "	6 " 32.9 "	—34.5	25.7	5 " 54.5 "	—40.9	23.1
			F–E.			
½ min.	31.4 secs.	+ 4.6	30.3	37 secs.	+23.3	22.7
1 "	1 min. 4.7 "	+ 7.8	7.4	1 min. 2 "	+ 1.6	6.9
2 "	2 " 19.4 "	+16.1	29.2	2 " 20 "	+ 1.6	21.8
4 "	2 " 57.6 "	—26	28.2	2 " 34 "	—30.5	29.3
6 "	5 " 52 "	— 3.3	11.6	6 " 1 "	+ 0	10.7
10 "	6 " 15.4 "	—34.1	19.4	5 " 49 "	—41.8	19.3

Difference due to Filling.

	Average.	Median.
½ min.	+10.6 per cent.	+44.9 per cent.
1 "	—10.2 "	+ 1.6 "
2 "	+41.6 "	+28.2 "
4 "	+15 "	+13.5 "
6 "	+35.2 "	+34.4 "
10 "	+ .4 "	— .9 "

Subject *R*, in Table IV., shows a positive difference, with the exception of the interval of one minute, where the median and average give conflicting results, and of the interval of ten minutes, where there is practically no effect.

We have in the tables four sets of figures that represent the effect of the filling on the estimates of *D* and of *R*. These figures are based on the average and the median of each of the two groups of experiments. I think we may safely infer that when the average and the median for any given interval of the same group have opposite signs, there is no clear effect due to the filling. The common result for these four modes of comparison would then be that the effect of the filling was to make the time seem shorter to *D* during the intervals of one, two, six and ten minutes. The two groups give conflicting results for the intervals of one half and four minutes, so that the position

of the filling—whether it came in the first or second of the intervals—was the more important factor. All four modes of comparison agree that to *R* the filled time seemed longer during all intervals except one half and one minute. In these two intervals the position of the filling is again the chief factor. In the case of the third subject, *S*, we have but one group of experiments. Here the filled time seemed longer at one minute but shorter at two, six and ten minutes. At one half and four minutes there seems to be no clear effect due to the filling.

It is evident from these results that the filling does not affect all three subjects alike. In general, the filled time seemed shorter than the empty to *D* and *S*, but longer to *R*, though there are exceptions in all three cases.

Table V.

Subject *S*. *E–E*.

Interval.	Av. Estimate.	D %	M.V. %	Median.	D %	M.V. %
½ min.	43.6 secs.	+ 43.6	20.4	40.5 secs.	+ 35	25.4
1 "	1 min. 6 "	+ 10	15.7	1 min. 2 "	+ 3.3	27.7
2 "	2 " 26.4 "	+ 22	27.7	2 " 47 "	+ 39.1	21.9
4 "	3 " 39.9 "	— 8.3	17.2	3 " 47 "	— 5.4	16.1
6 "	5 " 48.5 "	— 3.1	26.6	6 " 2 "	+ 3.3	24.8
10 "	10 " 10.6 "	+ 1.7	15.4	11 " 5 "	+ 10.8	5.9

F–E.

Interval.	Av. Estimate.	D %	M.V. %	Median.	D %	M.V. %
½ "	44.2 secs.	+ 47.3	21.3	39 secs.	+ 30	21.5
1 "	1 min. 40.9 "	+ 68.1	15.3	1 min. 44 "	+ 76.6	16
2 "	1 " 56 "	— 3.3	15.8	1 " 55 "	— 4.1	15.9
4 "	3 " 50.4 "	— 4	15.4	3 " 33.5 "	— 11.2	15
6 "	4 " 42.2 "	— 18.8	10.8	4 " 22 "	— 27.2	9.4
10 "	8 " 59.1 "	— 10.1	31.1	9 " 3.5 "	— 9.4	11.2

Difference due to Filling.

	Average.	Median.
½ min.	+ 3.7 per cent.	— 5 per cent.
1 "	+ 58.1 "	+ 73.3 "
2 "	— 25.3 "	— 43.2 "
4 "	+ 4.3 "	— 5.8 "
6 "	— 15.7 "	— 30.5 "
10 "	— 11.8 "	— 20.2 "

The estimates of empty times as compared with empty times, of the three subjects, as shown in Tables III., IV. and V., are in all respects comparable. If we consider only those intervals

where the average and the median are of the same sign, as decisive, we have for *D* no apparent error at one half or one minute, a positive error at two minutes, a negative error at four and six minutes, and no error at ten minutes.

The results do not show a constant negative error, such as was found by Michael Ejner for intervals of one half, one, two, three and four minutes marked off by sound. I found in taking the estimates that when a short interval followed a longer one it was in general lengthened. This fact may in part account for the overestimation of the shorter intervals.

When we compare the estimates of empty times of *D* and *R* with those of the first group, Tables I. and II., we find that *D* has lengthened his estimates in this second group (compare Tables I. and II.). This change was not, I think, due to practice so much as to the increased strain of attention demanded by the lack of knowledge of the probable length of the standard. *R* has decreased the estimate of one half minute, and, in general, made the estimates of Table IV. smaller than those of Table II.

For all intervals longer than one or two minutes my subjects expressed a dissatisfaction with their estimates and felt that they made little, if any, difference between the longer intervals—all times seeming very long and very much alike. *R*, at times, could not *consciously* note any difference between standards of two and six minutes, or between those of four and ten minutes, even when they followed each other in close succession, though her *results* show a constant and decided difference. *D* had a better idea; for, when asked how long he thought an interval had been, his verbal answer more nearly approximated the duration he had just marked off as 'equal' in the experiment. *S* entered the experiments with a general knowledge of the lengths of time that were to be used as standards, though ignorant during the experiments as to what particular one was being given him—but beyond two minutes could not with any constancy identify them and tell whether the standards had been four, six or ten minutes.

During the longer periods it was impossible to keep the attention so closely fixed as during the intervals of one half and

one, or at most two minutes. It is at about this point that the change of sign occurs in the estimates. The general feeling of weariness seemed to be the chief criterion in the longer intervals.

The difference due to the filling was, I think, merely a difference in the direction of attention, the monotonous regularity of the lights being, in general, a means of holding the attention and preventing the mind from wandering. From this point of view the filled time was psychologically the more empty or barren of the two—the time being filled with monotonous sensations of light, but empty of vivid or interesting trains of thought. In looking back over it, then, there would be fewer changes in consciousness to remember, and hence the time would seem shorter. This would be in keeping with the fact that the increased mental activity produced by certain narcotic drugs makes time seem long; the person, on recovery, remembering the vast number of his experiences, overestimates the time.

What is the result of these experiments as compared with Dr. Meumann's, and with the space illusions of sight and touch? We find that in sight a space is overestimated when it is filled; an interrupted line will seem longer than a continuous one, a line divided into more than two parts longer than an undivided one of the same length. In touch, while an interrupted line of 10 mm. is underestimated, yet a longer one, 10 cm., will be overestimated when it is interrupted.[1] Whether the effect of the filling in these time intervals corresponds to that in the space illusions would depend on which of the two times we consider to be the filled; for, in these long intervals the sensations of light have but an indirect influence, and are not the only filling, nor the chief factor in the appreciation of time. Taking it, however, as ordinarily understood, we do not find here a constant negative error such as Dr. Meumann found in his longest times, although to two of the subjects, *D* and *S*, the filled time in general seemed shorter.

The results of the third subject, *R*, are more in accord with the space illusion of sight, and of touch when the standard is one of 10 cm.—the filling making the stretch seem longer.

[1] See the accompanying paper by Miss Robertson, already referred to.

To test the question as to the effect of single and multiple divisions of time, and to determine whether in the temporal estimate there was anything like the space error in vision, where halving produces a negative error and more divisions a positive one, visual intervals of 3, 6, 12, 18, 30 and 60 seconds were, by the same method as that described above, divided into halves, thirds and fourths.

Under each interval ten estimates were taken for an empty time, and ten for each character of filling, on each of two subjects. A pause of two seconds was made between the standard and the compared time, the compared time being here always empty. In order that the subjects might know when the end of the standard had arrived, the word 'now' was spoken immediately after the last flash of the standard. A stop-watch measuring two tenth seconds was used by the experimenter to mark off the estimate. Although there was a reaction error here, yet it was common to both sets of experiments alike, and so might be neglected in comparing them.

TABLE VI.

SUBJECT *R*.

Interval.	No. of Lights.	Average.	M. V. %	Median.	M. V. %
3 secs.	2	3.94	10	4	9
	3	4.24	24	4	25
	4	4.71	15	4.7	14
6 "	2	6.28	16	6	16
	3	6.88	20	6.5	20
	4	8.82	16	8.4	16
	5	8.80	15	9.2	14
12 "	2	10.62	17	9.8	17
	3	10.96	13	10.4	10
	4	12.88	9	12.2	9
	5	13.38	8	13.8	7
18 "	2	13.24	16	13	15
	3	13.46	22	13.3	21
	4	16.44	13	17.3	12
	5	14.10	9	13.6	8
30 "	2	20.04	16	19.1	17
	3	20.22	23	19	20
	4	19.66	20	16.6	29
	5	24.58	26	23.9	22
60 "	2	32.84	26	28.7	26
	3	35.90	35	30.9	37
	4	39.76	19	40.1	19
	5	35.56	19	32.7	16

TABLE VII.

SUBJECT *Rd.*

Interval.	No. of Lights.	Average.	M. V. %	Median.	M. V. %
3 secs.	2	3.74	19	3.8	18
	3	4.56	13	4.5	13
	4	4.28	10	4.4	10
6 "	2	4.80	8	4.8	6
	3	6.68	17	6.9	16
	4	7.44	16	7.1	15
	5	8.36	12	8.7	12
12 "	2	10.07	14	10	14
	3	10.34	19	9.3	20
	4	11.11	22	9.9	22
	5	11.48	16	11.6	16
18 "	2	11.05	12	10.3	14
	3	13.83	11	14.35	9
	4	12.76	14	13.8	11
	5	15.35	12	15.3	10
30 "	2	17.41	14	16.3	14
	3	18.17	13	17.75	13
	4	17.99	18	16.35	16
	5	18.63	11	19.4	16
60 "	2	30.64	12	30.4	11
	3	34.56	16	34.3	17
	4	30.71	11	29.9	11
	5	33.54	17	34.2	16

The result we find (see Tables VI. and VII.) is that whether the average or the median be taken as the basis for comparison, the empty time seemed shorter than the filled, and, in general, the time seemed longer as the number of impressions was increased. For the three longer periods—eighteen, thirty and sixty seconds—the standard when divided into halves seemed longer than when divided into thirds to *Rd*; while to *R* the standard when divided into thirds seemed longer than when divided into fourths, during the intervals of eighteen and sixty seconds.

It is probable that in these longer periods the attention is not held closely to the sensations of light, so that other factors play a greater part in determining the estimate. In the shorter intervals, however, the mind can be kept relatively empty, so that the sensuous filling is the chief measure of duration. As long as the attention could be concentrated on the sensations, the number of lights would, I think, affect the estimate. Cases where in the present experiments the standard was subdivided

into thirds could always be consciously distinguished from those divided into halves, but many times where the standard was divided into fourths it could not be distinguished from thirds; more than fourths, I feel sure, could not have been apprehended without counting.

From these results we would say that in relatively short times as well as in spaces, the estimate is influenced by the number of impressions that fall within the stretch. There is no evidence whatever of a shortening up of the estimate due to the division of the standard into halves, such as is found in vision.

With *Rd* the division of the standard into fourths always gives a greater estimate than the division into thirds, but the estimate of thirds is often less than that of halves. *R*, with but three exceptions, increases the estimate as the number of divisions increases.

We find a great similarity in the absolute durations given in the estimate of the two subjects. Practice on longer intervals does not enable *R* to judge these shorter intervals any more accurately than *Rd*, who had no former practice, nor does it tend to reduce the variation; this being as large as in the former experiments and somewhat larger than that of *Rd*.

The position of the indifference point—where there is no absolute over- or underestimation—lies with both *R* and *Rd* between six and twelve seconds. The overestimation of three and six seconds may be due to assimilation with longer intervals. This would correspond with the results of Estel,[1] who found that an interval of three seconds, when it followed one of two seconds, seemed shorter than when it followed one of four seconds. This would also explain the shortening up of the estimates of empty intervals of thirty and sixty seconds in the case of subject *R* as compared with her former estimates given in Tables II. and IV.

As a final result of these experiments we find, in intervals of time ranging from three to sixty seconds, evidence of a temporal illusion very similar to the space illusion of sight. Both in time and in visual space, when there is more than a single division, the filled stretch is overestimated. We do not find,

[1] *Philosophische Studien*, II., p. 55.

however, that a single division shortens up the temporal estimate. This is in keeping with the space illusion of touch when the standard is 10 cm., but opposed to Dr. Meumann's results, as he finds intervals from four to nine seconds are underestimated when a single division is used.

As we increase the length of the standard interval to minutes, we do not get a direct reversal of the effect of the filling such as is found in touch; but we find the illusion either decreases or is entirely lost.

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STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF CALIFORNIA.

COMMUNICATED BY PROFESSOR GEORGE M. STRATTON.

VI. 'Geometric–Optical' Illusions in Touch.

BY DR. ALICE ROBERTSON.

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VI. 'GEOMETRIC–OPTICAL' ILLUSIONS IN TOUCH.

BY DR. ALICE ROBERTSON.

The interest which attaches to experiments upon the so-called 'geometric-optical' illusions, viz., the investigation of our perception of space, is not lessened when the investigation is carried into the tactual field. The experiments recorded in the following pages constitute an attempt to investigate, by touch alone, certain geometrical figures which present well-known optical illusions. Since sight and touch are so closely related, and since our theories of space perception are based in the main upon optical phenomena, the following observations may serve to test some of these theories. For example, from his study of reversible perspective, Thiéry[1] arrives at the conclusion that all optical illusion is due to the perspective in any given figure, whether consciously or unconsciously perceived. According to this observer, the convergence or divergence of lines produces in us an effect of depth, or of foreshortening, so that small angles are only larger ones interpreted perspectively, and an object seen near the apex of an angle seems larger than one at its opening, because it appears to be further away, and we connect distance with larger size. It is obvious that the tactual perception of plane figures, the mere contact of the fingers or of the hand upon any part of a flat surface, can produce no effect of perspective. When, however, it is found that illusion remains, serious doubt is cast upon the importance of perspective, even in the sight illusion.

In considering what figures are suitable for experimentation in the tactual field, it is clear that not all figures which pro-

[1] *Phil. Stud.*, XI., pp. 307 and 603, XII., p. 67.

duce an effect upon sight can be used. Simple figures, those containing but few lines, are best adapted to this purpose. If the figure is composed of many lines, a blur of sensations is received, and, as would be said in microscopy, it is difficult to get a sharp definition. The apparatus which was used in the following experiments consisted of black cardboard in which the figures were pricked with a fine cambric needle, the prickings being placed so close together that they could not tactually be distinguished as separate points. Or, in a few cases, the shape of the figure to be experimented with was made by pasting narrow strips upon a larger piece of cardboard, and this outline was either explored by the tips of the fingers, or the hand as a whole was passed over the figure.

Throughout these experiments, active touch has been employed. The rapidity of movement, the amount of pressure exerted, and the portion of the hand which received the sensation, make marked differences in some cases in the amount of illusion. Sometimes an illusion which existed in a very marked degree when the hand as a whole was passed over the figure, became almost inappreciable if the finger-tips were freely used to explore the contour. In other cases the illusion remained whichever method was adopted. As a rule, the hand has been passed rather lightly and somewhat rapidly over the figure, and the judgment has been recorded either in words or in a drawing of the object as it was perceived by the tactual sense.

The agreement or divergence between the illusions of touch and of sight afford a wide basis of classification for the experiments here described. In very few cases only can the tactual illusion be said to be merely in the same, or in a reverse direction from that which is found in sight. Other phenomena of illusion also appear, *e. g.*, illusions of curvature where lines are straight, or illusions of greater length or height where no difference exists in reality. Generally speaking, however, the whole set of experiments is divisible into two classes. The first includes those figures in which the illusion follows the same direction as that of sight. The second includes those figures which afford an illusion in the opposite direction. Other phenomena connected with these figures will be noticed in the description.

Class I.

The experiments which fall under the first class consist of a miscellaneous group of minor illusions which are common and well known in the field of sight. The purpose has not been to make a complete investigation of the phenomena revealed here, but merely to find out whether illusion exists, and, if so, to what extent it resembles the visual phenomena. These figures were presented from time to time to several subjects, no attempt being made, except in one or two cases, to vary the conditions.

1. *Müller-Lyer Illusion.*—In this well-known figure a marked tactual illusion exists. For purposes of experiment the oblique lines at the extremities of the horizontals were not joined close to the latter. Space enough was left so that the ends of the horizontals could be distinctly felt. In every case illusion

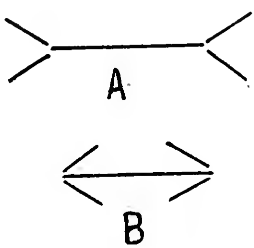

Fig. 1.

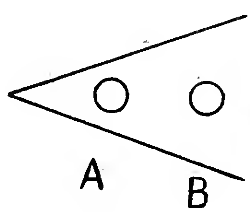

Fig. 2.

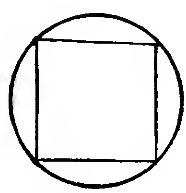

Fig. 3.

in regard to the length of the line was very apparent. It was in the same direction as that found in sight, but greatly intensified. That is, when compared by the sense of touch alone, *A* (Fig. 1) seemed not only longer than *B*, but the difference in length between them seemed much greater than appears to sight.

2. *Illusion of Convergent Lines.*—Experimentation upon a suitable figure of the pattern represented in the drawing gave perfectly definite and unvarying results. When the hand is passed over the figure and the sizes of the two circles are compared, that one (*A*) which is in the apex of the angle seems the larger—a result similar to that which is found in sight. The result in question seems to be due to a blending, to a certain degree, of the sides of the angle with the periphery of the circle and an interpretation of this as meaning that the circle *A* is larger than *B*, *B* being relatively uninfluenced by the lines

near it. If this be true, then the apparent size of *A* relative to *B* should change with a change in its position relative to the apex of the angle. This supposition seems to be confirmed by a few experiments conducted for the purpose of testing it.

3. *Perception of Angles.*—In the optical illusion presented by Fig. 3, the circle seems to be flattened somewhat where it touches the corners of the square, while the sides of the latter are very slightly bent inward. The same phenomena greatly accentuated appear also in the tactual illusion. In experimenting with this figure, subjects were requested not to explore the contour with the figure-tips. A record of the impression received by passing the hand back and forth over the figure as a whole, was made in drawing by each subject, and samples of the data obtained from two subjects, *S* and *N*, are given below (Figs. 4 and 5).

Fig. 4 represents the impression which subject *S* received

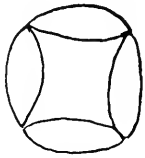

FIG. 4.

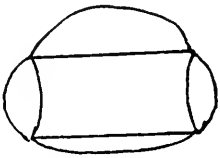

FIG. 5.

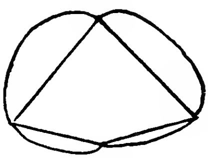

FIG. 6.

when the hand was passed over the figures as a whole in any direction. The sides of the square curved inward, and the periphery of the circle seemed to be divided into distinct segments, which flattened very much as their extremities approached the corners of the square.

Fig. 5 represents the impression received by subject *N* of the same figure. In this case the hand was passed from right to left or *vice versa* across the figure as a whole, at which time the square lengthened horizontally, the shorter sides only seeming to curve slightly inward. The circle seemed to be an ellipse somewhat flattened at the corners of the inner rectangle. When the figure was turned through 45°, and the hand was moved as before from right to left and back again, then the square became a flattened diamond shape, and the circle an ellipse somewhat flattened at the corners of the inclosed figure

(Fig. 6.) To this subject, 'horizontal' distances, that is, distances right and left, seem distinctly longer than equal vertical ones. Also, the upper part of a figure which is felt by the hand as a whole, usually seems higher and more distinct than the lower part of a symmetrical figure. Thus, in the two positions recorded above (Figs. 5 and 6) the curve of the ellipse is higher above than below, as is also the point of the diamond in Fig. 6.

4. *Illusions of Contour.*—A tactual illusion similar to that

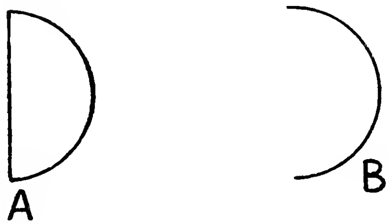

FIG. 7.

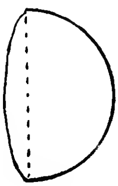

FIG. 8.

which is found in sight appears also when the hand is passed over two semicircles, the one closed and the other open, as represented in the drawing (Fig. 7). In this case the arc of the open semicircle seems to flatten out and to become the arc of a larger circle. Besides this illusion in contour another one appears in *A* which is not observed in sight. When the contour of *A* is perceived by the hand as a whole, the first impression is that of a figure composed of two curves, one of which is flatter than the other (Fig. 8). The curve of the arc of the circle seems to impress itself upon the chord and it appears to bulge slightly.

Similarly, if the two squares *A* and *B* (Fig. 9) are compared in the manner above described for the semicircles, the open figure *B* will seem the larger. In some cases *A* is felt as a square, *B* as a rectangle whose longer sides are horizontal. In other cases both *A* and *B* seem to be lengthened rectangles, *B* seeming the longer of the two. In the comparison of the semicircles and of the squares, the illusion in each case corresponds to what we find in sight, and probably for a similar reason, viz., the inclusion within the figure of some of the outside free space.

5. *Ring Segments.*—When the two segments (Fig. 10), which are objectively equal, are compared by touch, an illusion similar in direction to that of sight is very evident. Not only is a tactual illusion apparent when the segments are objectively equal, but also when the upper segment is actually larger than the lower, and when to sight no illusion whatever exists. That is, it is found by experimentation that, if two segments are compared in which the inner curve of the upper segment is equal to the upper curve of the lower (Fig. 11), a tactual illusion is apparent in a larger number of cases. In Fig. 10, 80% of the judgments obtained from five persons were in accord with the ordinary visual illusion, that is, *A* seemed smaller than *B*. At the same time, in the other 20% of the judgments, *A* was con-

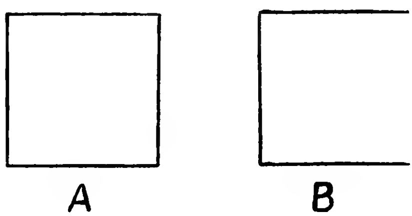

FIG. 9.

sidered either larger or equal to *B*, or the subject was in doubt. The evidence for illusion in this figure is by no means so conclusive, so unvarying in its effect on the tactual sense, as it is in vision. On the other hand, it is remarkable that in the unequal segments represented in Fig. 11, so large a percentage of judgments should give evidence of a tactual illusion. The larger size of *A* in Fig. 11 is very evident to the eye, yet when the comparison is made by touch, in 42% of the judgments *A* is considered smaller or equal in size to *B*. If the cards are turned at right angles, and the segments are compared in this position, the errors in judgment are increased. In the case of Fig. 10, the increase is not large, 81% of the judgments are in favor of the smaller size of *A*, while for Fig. 11, 56% of the judgments are that *A* is either smaller than *B* or equal to it. The error in these figures seems to indicate that the tactual comparison of the two segments becomes a comparison of the lengths of the two more closely approximated curves, rather

than a comparison of the size of the segments as a whole. This is thought to be the reason why an increase of errors occurs when the cards, and consequently the segments, are turned at right angles to the positions represented in Figs. 10 and 11. In this position the oblique sides of the segments are brought directly under the fingers, and hence come into more prominent notice. The tips of the fingers naturally follow the slanted edge of the upper or right-hand segment, and thus they are brought some distance within the slanted edge of the lower, or left-hand segment, and the former is considered the smaller.

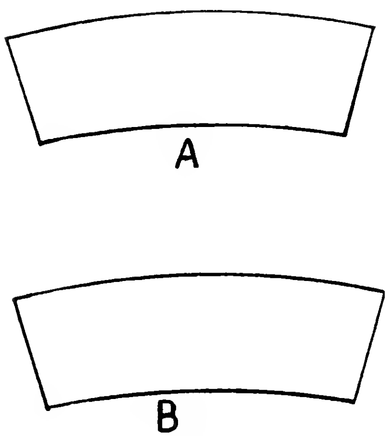

FIG. 10.

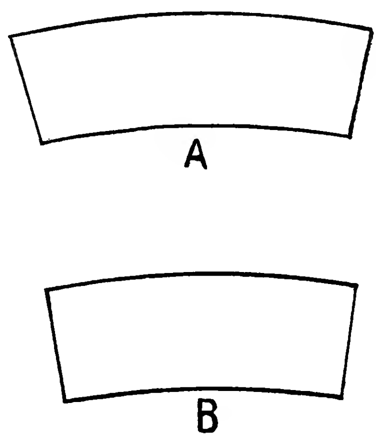

FIG. 11.

In each of the preceding figures certain tactual illusions occur which are in the same direction as those which appear to the eye and which seem analogous to the optical illusions. As has been said, these optical phenomena have been ascribed to perspective as the primary cause; but in the experiments here presented perspective cannot enter, and yet the results are the same. While it does not necessarily follow that the phenomena of sight and touch should be referred to the same cause, yet the results here obtained are thought to diminish the force of perspective as a fundamental cause of illusion even in sight.

Class II.

1. *A Quantitative Comparison of Lines of a Varying Number of Interruptions with an Uninterrupted Line of Standard Length.*—The experiment here described is intended to investigate the phenomena which appear when lines variously interrupted are compared, by the sense of touch alone, with an uninterrupted line. The results show a reversal of the illusion which appears in the visual field, and instead of an overestimation of the interrupted extents such as takes place in vision, there is underestimation, *i. e.*, the interrupted lines seem shorter than they actually are.

The apparatus which was used consisted of a number of cards, made of black cardboard, 27 cm. long by 13 cm. wide, in the center of which the lines were pricked. Four kinds of lines were used. First, a plain, uninterrupted, smoothly pricked line (————). Secondly, a line similar to the preceding but having its extremities defined by a short line at right angles (|————|). Thirdly, a line defined at each extremity by cross lines and also divided in the middle (|——|——|); and fourthly, a line divided in a similar manner into sixths (|–|–|–|–|–|–|). The cards thus fall into four groups, each group consisting of a series of lines varying in length from 7 cm. to 15 cm. The lines varied by steps of 5 mm. throughout that portion of the series where the difference was actually found to be difficult to perceive. The lengths of the lines composing a series, then, were as follows: 7, 8, 9, 9.5, 10, 10.5, 11, 11.5, 12, 12.5, 13, 14, 15 centimeters. The standard line with which all comparisons were made, was a plain unbroken line (————) ten centimeters in length.

In conducting the experiment the subject was seated with closed eyes before a table of convenient height, upon which his whole arm rested comfortably. The standard card was first presented to him, and then the other cards of any particular set, one after the other, were laid below it for his comparison. He was permitted to pass the hand back and forth from the card to be compared to the standard, and *vice versa*, as often as he pleased before he gave his judgment. The right hand was always used, and no restrictions were placed upon him as to

what part of the hand should receive the stimulation. Sometimes the tips of the fingers alone were used, again the portion of the hand just below the finger-tips. In giving a judgment the subject was asked to state whether the line seemed longer or shorter than the standard or equal to it. A series consisted of an ascending and descending portion, and an equal number of each began with the shortest line and proceeded gradually to the longest, and *vice versa*.

Ten such double series were obtained from each of three subjects, *B*, *S* and *N*, the results of which are shown in Table I. As a matter of fact, series were obtained from many more persons, and the results in many cases were much more striking than those which are here presented. The experiments here recorded are, however, in every way the most systematic and trustworthy. In the table each value for the upper (U. T.) and lower (L. T.) threshold is an average of twenty single determinations, and the equality point (E. P.) and mean variation (M. V.) are an average of forty determinations.

Examination of the table of thresholds shows much individual variation in the ability to estimate the differences between the various lines. All show underestimation, that is, the line which is compared, when really equal to a standard, seems shorter than the standard, even in Group I. There seems to be much difficulty for all subjects to discriminate in the case of this group. In the

Table I.

Table of Tactual Thresholds and Mean Variation for the Four Groups of Lines.

	Group I.				Group II.				Group III.				Group IV.			
	U. T.	L. T.	E. P.	M. V.	U. T.	L. T.	E. P.	M. V.	U. T.	L. T.	E. P.	M. V.	U. T.	L. T.	E. P.	M. V.
B	10.4	10.4	10.4	.12	10.9	10.8	10.8	.25	11.3	11.2	11.2	.28	11.4	11.2	11.3	.43
S	11.2	10.8	11	.24	12.3	11	11.6	.43	12.1	11.6	11.8	.37	12.5	11.8	12.1	.53
N	10.7	10.6	10.6	.40	11.8	11.7	11.7	.34	11.2	11.2	11.2	.37	12.6	10.5	10.5	.38

cases here reported one subject, *S*, shows an underestimation amounting to 1 cm., the other two subjects each average about one half a centimeter of error. The amount of error in the case of subject *S* is always large, but increases at an even rate with the number of interruptions, so that the line with the greatest number of interruptions seems in his case to be the shortest. For subject *B* the amount of underestimation is somewhat less in each case, but it proceeds at the same even pace, and a line much divided seems shorter than one objectively equal but undivided. The case is somewhat different for *N*. For this subject the effect of the limiting lines at the extremities in Group II. is marked by a sudden increase in the amount of underestimation. The compared line in Group II. seems to be shortest of all, while the compared lines in Groups III. and IV. relatively lengthen. But even with this subject the line containing the greater number of interruptions is equal to the uninterrupted line and not longer, as is the case in sight.

From the data furnished by these experiments, we may conclude that when a line ten centimeters in length, definitely marked at its extremities, and with or without interruptions in its length, is compared with a plain unbroken line objectively equal to it, it appears shorter to the tactual sense, or is underestimated. In general, this result agrees with the conclusion at which Professor Parrish[1] arrived in his investigation of similar phenomena with passive touch. He used lines 64 mm. long, all being marked at their extremities and variously interrupted in their extents. He considers that the results which he obtained clearly point to a reversal of the optical phenomena. Dr. Dresslar,[2] on the other hand, concludes from experiments which he conducted with both active and passive touch, that the tactual illusion follows the same direction as the illusion of sight. A study of the data of the latter's experiments, however, given in Tables I. and II., pp. 334, 335, of his article, suggests that perhaps a transition-point from under- to overestimation may be found in them, between the long and the short

[1] *Amer. Jour. of Psy.*, VI., p. 514.
[2] *Amer. Jour. of Psy.*, VI., p. 314.

interrupted intervals. Certainly in Table II., in which the judgments are given upon longer lines (5 to 16 cm.), there is a decided falling off of the relative number of judgments in favor of the greater length of the filled space. Indeed, the writer himself remarks on page 337, that 'when the spaces to be compared are more than 10 cm. in length, the illusion does not hold so steadily.' In fact, from about 10 cm. on, the illusion tends to take the opposite direction from that which appeared below that length and from that which appears in the visual field.

From the results of a few tentative experiments upon short interrupted intervals, an analogy between our sensations of touch and our perception of time is suggested. It is well known that time of a given length, but interrupted at regular intervals, seems within certain limits to be shorter than an equal unbroken period. It has been found, however, that for very short intervals the illusion changes in character, and such periods when interrupted at regular intervals appear to be longer than an equal unbroken time.[1]

For the purpose of investigating this matter experimentally a number of cards were prepared, on each of which there was marked off a short space defined by limiting lines. The spaces formed a series and were respectively 8, 9, 10, 11 and 12 mm. wide, defined at each extremity by a pricked line one centimeter in length. The standard for comparison consisted of a space 10 mm. wide which was broken at regular intervals by five lines (| | | | |). Thirty series (150 judgments) were obtained from each of the three subjects, *B*, *S* and *N*. The method of right and wrong cases was adopted. The cards to be compared were presented in no regular order, but were shuffled at intervals. A parallel experiment, thirty series for each person, was also carried out, the standard in this experiment being an unbroken space of 10 mm. long (| |).

The results of the two experiments are given in Table II. The data for both experiments from each subject are placed one below the other so that their comparison may be more easily made. The upper line of the table gives the widths of the unbroken spaces, or the variables which were compared with

[1] Meumann, *Phil. Stud.*, IX., p. 264, and XII., p. 127.

Table II.

Sub.	No. of Exp.	Character of Standard.	Distribution of Judgments when Variable was:																									
			8 mm.					9 mm.					10 mm.					11 mm.					12 mm.				
			L.	S.	E.	E. or S.	D.	L.	S.	E.	E. or S.	D.	L.	S.	E.	E. or S.	D.	L.	S.	E.	E. or S.	D.	L.	S.	E.	E. or S	D.
B	30			30					30					28	1	1		2	16	12			10	8	11	1	
	30			28	2			2	18	4	1	4	7	11	5	2	4	25	1	4			28	1	1		
N	30		1	27	2			1	26	3			9	17	4			18	9	3			30				
	30		1	29				2	19	9			15	5	10			29		1			30				
S	30		6	19	5			5	17	8			9	11	8		2	17	8	4		1	24	1	3	2	
	30		1	23	5		2	2	19	7		2	7	10	8		5	24	1	5			29			1	

the two standards. The letters L, S, E, E or S, and D in the second line, stand respectively for the judgments longer, shorter, equal, equal or shorter, and doubtful. The number of judgments of each kind is arranged in two lines for each subject, the upper line giving the judgment of comparison with the broken standard (| | | | |), the lower line, those with the unbroken one (| |). Thus, taking the first two lines which represent the judgments given by the subject *B*, we see that an unbroken interval of 8 and 9 mm., when compared with the standard, is thought to be shorter each time. The comparison of an unbroken interval 10 mm. in length with an equal broken interval gives 28 judgments of shorter, and the unbroken spaces of 11 and 12 mm. give a predominance of the judgments of shorter and equal. Contrasting these results with those obtained from comparison with the unbroken standard, it will be seen that for this subject there is ample evidence for an overestimation of intervals where the standard is 10 mm. in length. In these sets the 10 mm. and 11 mm. intervals are the most instructive. Considering those for subject *N*, the 10 mm. unbroken interval is considered shorter than an equal broken interval 17 times, and equal only 4 times; while the 11 mm. interval is thought to be longer 18 times, and either shorter or equal 12 times. When the same two intervals are compared with the unbroken standard, the judgments of shorter for the 10 mm. interval diminish, while those for 11 mm. show almost no illusion.

In the case of subject *S*, the results are not so conclusive. Unlike the first two subjects, he knew the purpose of the experiment and felt, himself, that this knowledge was a difficulty in the way of giving a ready judgment. When the unbroken standard was used for comparison, there is a slight decrease in the judgments of 'shorter' and an increase of 'doubtful' and 'equal' for the 10 mm. interval; while for 11 mm. there is a decided increase of judgments of 'longer' and 'equal' with a decrease of 'shorter.' These results, when considered by themselves, may be said to indicate a tendency toward the overestimation of interrupted intervals. Taken in connection with those given by the two other subjects, there is a strong indica-

tion that in the tactual field a general law holds true, viz., that long interrupted extents are underestimated, short ones overestimated.[1]

The underestimation of interrupted extents by the tactual

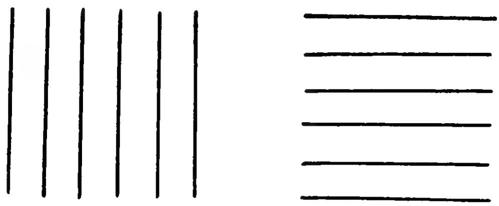

FIG. 12.

sense is also shown in the comparison of squares which are composed of either horizontal or vertical lines. When squares similar to *A* and *B* (Fig. 12), whose sides are 10 cm. long, are pricked in cardboard and are felt by running the hand as a whole over them from right to left, or vice versa, then an illusion appears in the reverse direction from that perceived by sight. *A*

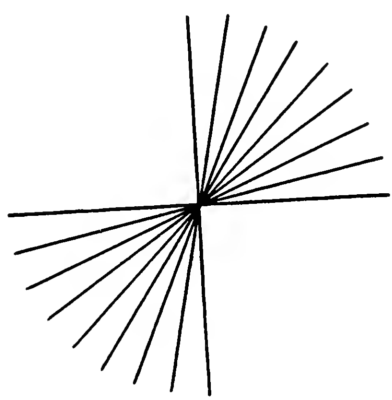

FIG. 13.

seems to lengthen so that it appears to be a rectangle whose horizontal sides are longer and whose vertical sides are shorter. On the other hand, *B* seems to shorten horizontally and to lengthen vertically. Similar phenomena occur in the figure which

[1] This is in accord with the results of Professor Rieber, to be published in the forthcoming volume of *Harvard Studies*. Dr. Rieber has very kindly placed his results in outline at our disposal for comparison.

represents alternate quadrants of interrupted and uninterrupted extents (Fig. 13). When the hand is passed over such a figure, the open or uninterrupted quadrant seems decidedly the larger. While this may be taken as added proof that interrupted extents are underestimated, yet the apparently very large size of the open quadrant is probably due in part to the inclusion by the hand of much of the surrounding free space. The arc through which the hand sweeps in passing over the open quadrant, not being well defined, seems greater and may in reality be greater than that through which it passes when feeling the quadrant filled with radiating lines. The tactual illusion in this case is analogous to that which is found in sight, although in an opposite direction. For, while in sight the uninterrupted quadrant seems smaller than the filled one, this is doubtless partly due to the fact that we compare the *arc* of the 'filled' quadrant, *i. e.*, the ends of the radiating lines, with the *chord* of the arc of the adjoining open quadrant. The optical illusion, then, is partly due to the *leaving out* of some of the space which belongs to the open quadrant. The tactual illusion, on the other hand, is heightened by the *taking in* of additional space.

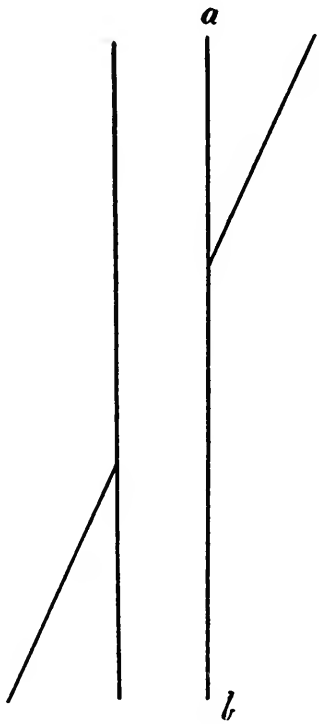

FIG. 14.

2. *Poggendorff Figure.*—The Poggendorff figure has long been a favorite subject for investigation as an optical illusion. Many theories center about it and the closely related Zöllner figure, but so far as I know no attempt has been made to investigate the tactual phenomena connected with it. A few tentative experiments gave very constant and somewhat surprising results. For whether the subject was or was not already acquainted with the optical illusion which appears in this figure,

whether he had or had not previously seen the figure which was presented to him, the illusion was very apparent, but in a reverse direction from that which appears to the eye. In the optical illusion which this figure presents, the lower left half of the oblique line appears to be *too low*, and not directly continuous with the upper right half (Fig. 14). In the tactual illusion, on the contrary, the lower left half of the oblique seems *too high* to be considered a continuation of the upper right half of the same line.[1] Moreover, the amount of displacement in the tactual illusion seems to be much greater than in the visual. It was, therefore, thought worth while to conduct a set of experiments for the purpose of making some quantitative determinations. A figure was constructed having one part of the oblique line movable, so that the amount of displacement could be easily measured. The verticals were placed 30 mm. apart, and the oblique crossed them at an angle of 40°. At no time was the subject permitted to see the figure. The sheet of cardboard upon which it was constructed was placed before the subject so that his arm was at right angles to the transverse line over which his hand should pass. It was found that if this line were slowly and carefully traced with the finger-tips, the illusion either did not appear or was very faint. In all cases it was required to judge of the direct continuation of the two parts of the oblique line by passing the flat of the hand over it, either alternately up and down, or in one direction only, as the subject desired. As a matter of fact, most persons settled into the one method of passing the hand from right to left downward over the line.

The experiment was conducted with four persons, *B*, *S*, *D* and *A*. From each of these, five double series were obtained. A descending series began with the transverse lines really continuous, although in no case did they seem so to the subject.

[1] Professor Dresslar proposes an explanation for an illusion of displacement of crossed lines which he considers may explain the optical illusion in the Poggendorff figure, and which by implication, at least, seems to be intended as an explanation of the tactual illusion in this figure. In my own experiments with the Poggendorff figure, the tactual illusion is shown to be in an opposite direction from that which appears in sight, and this fact would seem not only to render Dr. Dresslar's proposed explanation inadequate for the illusion in touch but also to throw doubt upon that offered for sight. See *Amer. Jour. of Psy.*, VI., p. 275.

The movable side was then moved downward by steps of 2 or 2.5 mm. to the point where the two halves of the transverse line seemed to the subject to be continuous, and then below that

TABLE III.

AVERAGE THRESHOLDS AND MEAN VARIATION.

Subject.	Upper Ave. Thr.	Lower Ave. Thr.	General Average.	Mean Variation.
B	— 15.2 mm.	— 24.1	— 19.6	4.6
S	— 27.2	— 30.1	— 28.6	8.3
D	— 24.0	— 30.1	— 27.1	4.5
A	— 45.0	— 50.3	— 47.7	9.0
Aver.	— 27.8	— 33.6	— 30.7	6.6

point until the left side was clearly too low. An ascending series retraced these steps to zero. Every such series, of course, gave two thresholds. In tabulating the data a calculated equality point was found of all the upper thresholds for the upper limit of continuity, or upper threshold. In a similar way the lower limit of continuity was found. Table III. gives the results which were obtained from each of the subjects according to this method. In the table the minus sign signifies the distance of displacement downward, measured along the line *ab* in Fig. 14.

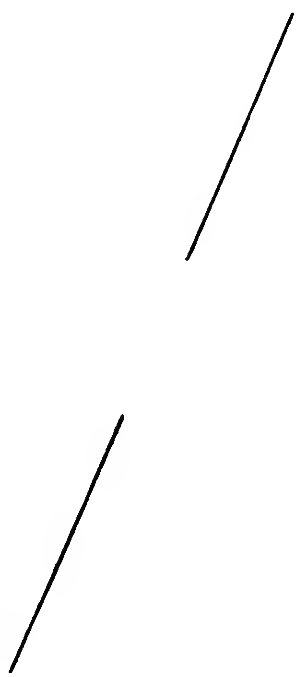

FIG. 15.

It will be seen that the mean upper threshold for all four subjects is — 27.8 mm., the mean lower — 33.6 mm., thus giving a general average of about — 31 mm. That is, the lower left-hand portion of the transverse line must be moved downward 31 mm. on an average before the two halves seem to be continuous. If we contrast this number with that which was obtained by Burmester[1] in his investigation of the optical illusion in the Poggendorff figure, we find a very

[1] *Zeitschrift für Psychologie*, XII., p. 369.

wide difference in the amount of displacement which the two senses of sight and touch reveal. With a breadth of 30 mm. between the verticals and an angle of 40°, this investigator found an average of — 5.09 mm. as the amount of displacement which was required to make the lines look continuous when the figure was in a vertical position.

With some persons the two halves of the oblique line felt as if they were parts of parallels, but the lower left-hand portion seemed to be at a higher level than the upper right-hand portion. It seemed to be the unanimous opinion of those who experienced this illusion that the feeling of 'too high' was due in large part to the vertical parallels. These lines, it was thought, guided the hand downward, below the point where it should cross the space between the verticals, and in order to reach the lower portion of the transverse line an actual upward effort was necessary. In order to test the influence of the verticals upon the illusion a second figure was made, omitting the vertical lines altogether (Fig. 15). This, like Fig. 14, was made with one side movable, so that the amount of displacement could be measured. Five double series were obtained from each of three subjects, *B*, *S* and *D*, and the upper and lower limits noted as before. The data are tabulated in Table IV., and some very interesting results appear. Thus, in the

TABLE IV.

TABLE OF AVERAGE THRESHOLDS AND MEAN VARIATION WHEN VERTICALS ARE OMITTED.

Subj.	Aver. Upper Thr.	Aver. Lower Thr.	General Average.	Mean Variation.
B	+ 5.3	+ 2.6	+ 4.0	3.95
S	— 7.2	— 8.3	— 7.7	3.49
D	+ .7	— 3.7	— 1.5	2.5

case of *B* the direction of illusion changes, and the lines seem continuous at some point above where they really are so. The judgments given by subject *D* vacillate above and below the zero point, and if we take the average of the two thresholds to be the point where the two lines would seem continuous to this subject, we find it to be — 1.5 mm. In this case, then, the illusion is practically nothing. With subject *S* the threshold

always falls below zero, on an average -7.7 mm. From the data it seems clear that in the absence of the verticals the tactual illusion is very greatly weakened and almost nil. In experimenting with a similar figure, Burmester found that the optical illusion was much weakened and took an opposite direction. This experimenter was at the same time his own subject, so that it is possible that if he had operated with other persons, individual differences would have appeared as they do here.

An attempt was made to counteract the influence of the verticals by filling in the space between the end of the oblique lines with lines running horizontally. The vertical parallels are of course suggested by the ends of the horizontals, but since the lines in the transverse direction are the more prominent, it was thought that they would exert the greater influence and weaken the illusion, or perhaps reverse its direction. Several figures were made, in all of which the inclination of the oblique line remained constant, 30°, but in which both the lengths of the horizontal lines and their distance apart varied. The results of experimentation indicated a decided weakening of the illusion, but in no case was reversal obtained.

In whatever position the Poggendorff figure in its normal form was laid, illusion was apparent. The amount of pressure exerted, and the rapidity of movement, seem to have an effect upon the amount of apparent displacement. Thus, in the case of a figure in which the oblique lines were fixed, it was found that, with hard pressure and rapid movement, the lower left-hand line seemed too high; whereas, with the same pressure approximately, and slow movement, the two halves of the oblique line seemed to be continuous.

Various theories suggest themselves as a partial explanation of the tactual illusion which is exhibited by the Poggendorff figure. That the verticals in some way influence the amount and direction of illusion in both sight and touch is obvious enough. To some persons they seemed to exert a mechanical influence in actually leading the hand astray, so that in passing downward from the upper right-hand oblique to the lower left, an upward effort is necessary in order to find the lower part of the line,

leading the subject to consider that that portion of the line is on a higher level. An attempt was made to get tracings of the path which the hand described in passing from one portion of the oblique to the other. This was done by placing strips of smoked paper in the path of a wire which was attached to the hand. Thus, in one instance when the lower oblique was moved downward 10 mm., in passing the hand from above downward the two halves seemed continuous, while in passing from below upward the lower left part of the oblique seemed too high; in these two instances, however, no difference can be detected in the two tracings. That part of the curve which represents the path of the hand between the verticals is almost a straight line in both cases, and each is the normal and regular continuation of the first part of the tracing. In another instance, the two obliques were separated by a vertical distance of 18 mm. To the subject the lines seemed continuous with both the upward and downward movement, and the smoked paper tracings were two perfectly even and smooth, almost parallel lines. In a third instance the obliques were separated by a vertical distance of 23 mm. At this point they seemed continuous to the subject, while the tracing shows many irregularities. These, however, occur, not only in the space between the verticals, but throughout the lines, and may be ascribed to natural tremors of the hand. There is no evidence that there is an actual upward movement of the hand corresponding to the effort which some subjects believed they felt.

Data obtained from figures similar to those used in these experiments have afforded a basis for opposing theories of space perception. The perspective theory of Thiéry has already been mentioned. He sees in the Poggendorff figure also a definite perspective effect which is the cause of the apparent shifting of the two halves of the oblique line. Professor Wundt[1] considers that the cause of the optical illusion in this figure is the overestimation of the acute angles. The perspective effect, he maintains, appears only when one fixates a point monocularly, at which time the displacement of the oblique

[1] 'Die geometrisch-optischen Täuschungen,' *Abhandl. d. königl. sächs. Gesellsch. d. Wissensch.*, XLII.

lines disappears entirely. The 'ästhetisch-mechanische' theory of Lipps,[1] offered first as an explanation of spatial form, has later been applied to geometrical optical illusions. Among other figures, this writer discusses the Poggendorff figure. He applies to it his theory of the interaction of opposing forces, and considers that it suggests the action of the two forces of gravity and vertical extension. The oblique line represents a force approaching, but not attaining, verticality. In the struggle this force is regarded as the primary activity, and as primary activity is overestimated.

We have, then, at least three explanations of the phenomena of the Poggendorff figure. It is here shown how the same figure may give very different sensations to the skin. How are the facts to be reconciled? If the optical phenomena of this figure are due to the overestimation of the bending of the obliques away from the vertical, according to Lipps, or if they are due to an overestimation of the small angles, according to Wundt, why should not these causes operate in the field of touch, and, if they do, why should opposite effects be produced upon the tactual sense? Likewise, no explanation for the optical illusion in the ring segments satisfies the touch phenomena in the same figures. Here again, perspective effects and the overestimation of small angles are offered in explanation. But the illusion persists in touch, when none is apparent to sight, and when all perspective and almost all angle effect is lost completely. The data which are afforded by experiments in the tactual field suggest a revision of the theories so far offered for spatial illusions in general. These theories are in the main founded upon optical phenomena. From what appears in the tactual field it is reasonable to suppose that further study may assist in elucidating this very complex and difficult problem.

[1] 'Raumästhetik und geometrisch-optische Täuschungen,' *Schriften der Gesell. für psy. Forschung*, Vol. II., p. 295.

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WITH THE CO-OPERATION FOR THIS SECTION OF

FROM THE UNIVERSITY OF CALIFORNIA PSYCHOLOGICAL LABORATORY

COMMUNICATED BY G. M. STRATTON

The Effect of Verbal Suggestion upon the Estimation of Linear Magnitudes

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the lower one being 2.2 cm. in width, and the upper one 1 cm. in width, having behind them backgrounds of the same blackness as the screens. Through the lower slot and against this background were exposed the printed slips bearing the mottoes, resting on a wooden ledge fastened to the back of the screen just below the edge of the slot. On a similar ledge behind the upper slot were exposed two white pegs .8 cm. in diameter and long enough to have their ends hidden by the screen. These pegs could be moved along the ledge, and hence adjusted for various intervals between them. On the near side of the nearer screen and just below the top, was a ledge on which the subject moved little pegs similar to those just described, and thus reproduced his estimation of the standard interval.

Over the front of the farther screen was made to slide a movable screen having a single horizontal slot through which either of the two slots in the stationary screen could be exposed, but not both together. Thus the subject saw either the motto in the lower slot, or else the standard distance marked off by the two white pegs appearing as rectangles 1 × .8 cm. in the upper slot, or else both slots were altogether hidden. The distance from the subject's eye to the nearer screen was 40 cm., and from the nearer screen to the farther was 80 cm.

The mode of operating the apparatus was sufficiently simple. The proper motto being in the lower slot, and the pegs having been properly spaced in the upper, and both slots being hidden by the movable screen, the operator, after due warning to the subject, raised the screen one notch and exposed the motto for two seconds; then raised it a second notch and exposed the standard interval for two seconds; then dropped it one notch and left the motto again exposed. The subject immediately so placed his pegs on the ledge as to mark off his estimate of the space interval, and the operator recorded the length so marked, reading it from a scale hidden from the subject's vision by a narrow strip of black paper projecting above the ledge.

1. First Set of Experiments.

In this first work only two subjects were engaged and a variety of suggestion mottoes were used, viz., '*Make short enough*,

'*Make long enough,*' '*Don't make too long,*' '*Don't make too short,*' '*Make short,*' '*Make long,*' a nonsense motto '*Zwp fjvic bgzx asye,*' and a meaningless sentence '*Life is real where.*' These mottoes were printed in black capitals 1.2 cm. high on white cardboard.

In groups *A*, *B* and *C* four standard lengths for estimatio were used, 16, 22, 28 and 34 cm. In group *D* standards of 24, 26, 30 and 32 cm. for subject C. and 12, 14, 18 and 20 cm. for subject Y. Long standards were used for subject C. in this group because from examination of results in the preceding groups it seemed that the long standards would give more definite results; and since standards for this subject were changed, it was thought advisable to change standards for the other subject also. Thus any possible difficulty from too long use of the same standards was obviated. The four standards employed in any group were given in succession with each motto, in an order determined by lot, the order of succession of the mottoes being changed for each day. A 'nonsense motto' was used in order that the tests without suggestion might be under conditions as like as possible to the others, except for the suggestion itself.

Tables I. and II. give in detail the results of this investigation. Instead of finding the average lengths reproduced with different mottoes it was deemed simpler to interpret the data by aggregation. The number tabulated under a given motto for a given day is therefore the sum of all the judgments taken that day for that motto. As remarked above, the series in both tables are arranged in four groups, which differ amongst themselves in regard to the mottoes and standards used, and also in that a month's interval elapsed between the work of group *C* and that of group *D*. For convenience in examining the tables, totals are given under each group for such columns as it is desirable to compare. As all the mottoes are not used on all the days, only such days are included in forming the totals as make the totals in the same horizontal line properly comparable.

An examination of these tables leads to some interesting conclusions. First, we find by comparison of daily totals and general totals, that the suggestion produces a definite, though slight, effect. The results for 'Make short enough' are in both

Table I.

Based on 650 Judgments; Subject, Miss C.

	1	2	3	4	5	6	7	8	9	10	11
	Day of Group.	No. of Judgments Each Motto.	Standard (Sum).	Nonsense Motto.	"Life is Real Where."	"Make Short Enough."	"Don't Make Too Long."	"Make Long Enough."	"Don't Make Too Short."	"Make Short"	"Make Long."
Group *A* (Eight different standards, 11–30 cm. long, were used in this group)............	1	9	189	199		—	212.7	—	—		
	2	8	167	179		—	—	180.7	—		
	3	10	202	221.5		227	225	223.5	224		
	4	10	202	218		224.5	214	220	223		
	5	10	202	—		220	221.5	216	221		
	6	15	320	343.6		—	341.5	—	342.9		
Sum of days 3, 4, 5		30	606	—	—	671.5	660.5	659.5	668		
3, 4, 5, 6...		45	926	—	—	—	1,002	—	1,010.9		
3, 4, 6...		35	724	783.1	—	—	780.5	—	789.9		
Group *B* (Standards, 16, 22, 28, 34 cm.)..	1	11	248							259.2	258.3
	2	16	322							302.5	305.2
	3	24	588							584	597.6
	4	24	660							650.7	680.3
Sum......		75	1,818							1,796.4	1,841.4
Group *C* (Standards, 16, 22, 28, 34 cm.)...	1	8	200	216.6	205		210.2		211.7		
	2	9	210	196.2	202.1		196.3		205.4		
	3	9	204	200.5	198.2		194.3		202.1		
Sum......		26	614	613.3	605.3		600.8		619.2		
Group *D* (Standards, 24, 26, 30, 32 cm.)...	1	6	170	—	—		183.2		175.3	172	185.5
	2	10	276	279.1	279.1		284.6		280.5	279	279.6
	3	7	206	209	211.6		221.7		213.6	203.3	212.5
	4	6	162	165.6	157.3		165.2		156.2	164.5	170
S. of days 1, 2, 3, 4		29	814	—	—		854.7		825.6	818.8	847.6
2, 3, 4...		23	644	653.7	648		671.5		650.3	646.8	662.1
Total, *C* & *D* (except day 1)......		49	1,258	1,267	1,253.3	—	1,272.3	—	1,269.5		
Total, *A* (except days 1, 2) *C* & *D*		100	2,354	—	—	—	2,457.5	—	2,455.7		
Total, *B* & *D*......		104	2,632	—	—	—	—	—	—	2,615.2	2,689

cases greater than those for 'Make long enough'; the results for 'Make long' are greater than those for 'Make short' for subject C., and also greater for subject Y. in group *D*, although less in group *B* for this subject. Similarly, results for 'Don't make too long' are pretty uniformly greater than those for 'Don't make too short' for subject Y., and also for subject C. in group *D*, but not for this observer in groups *A* and *C*.

TABLE II.

BASED ON 598 JUDGMENTS; SUBJECT, MR. Y.

	1	2	3	4	5	6	7	8	9	10	11
	Day of Group.	No. of Judgments Each Motto.	Standard (Sum).	Nonsense Motto.	"Life is Real Where."	"Make Short Enough."	"Don't Make Too Long."	"Make Long Enough."	"Don't Make Too Short."	"Make Short."	"Make Long."
	1	9	189	176		—	176.5	179.7	—		
Group *A*	2	10	202	188		197	188	192.5	190.5		
(Eight standards	3	10	202	186.5		191	190	194.5	186.5		
11–30 cm. long).	4	15	320	297.6		289	293.8	—	289.2		
Sum of days, 2, 3, 4		35	724	672.1		—	671.8	—	666.2		
2, 3...		20	404	374.5		388	378	387	377		
	1	21	534							503.3	487.8
Group *B*	2	22	538							499.9	491
(Standards 16, 22,	3	19	514							499.8	500.9
28, 34)...............	4	20	524							487.2	488.5
Sum...........		82	2,110							1,990.2	1,968.2
Group *C*	1	11	320	283.9	276.8		285.1		287.8		
(Standards 16, 22,	2	11	278	252.5	248.8		258.5		255.6		
28, 34)...............	3	9	198	176.5	178.1		175.1		172.2		
Sum...........		31	796	712.9	703.7		718.7		715.6		
Group *D*	1	6	98	94.7	91.7		92		94.3	92.2	92.8
(Standards 12, 14,	2	6	90	86.6	86.3		89.8		88.1	85.6	92.4
18, 20)...............	3	6	102	94.9	94.3		94.3		91	91.4	95
Sum...........		18	290	276.2	272.3		276.1		273.4	269.2	280.2
Total, *C* and *D*.....		49	1,086	989.1	976		994.8		989	—	—
Total, *A* (except day 1) *C*, *D*......		84	1,810	—	—		1,666.6		1,655.2	—	—
Total, *B* and *D*.....		100	2,400	—	—	—	—	—		2,259.4	2,248.4

Since group *B* followed group *A* and group *C* followed group *B* without gap, but a month elapsed between groups *C* and *D*, and since the difference in the presumable effects of the same mottoes occurs only between group *D* and the other groups, we must conclude that the uniformity is too great to admit of an explanation except by potency of the suggestion from the mottoes.

Second, this suggestion-effect varies both according to the individual to whom the suggestion is made, and also according to circumstances. The disagreements just referred to as existing between group *D* and the preceding groups is evidence upon the

latter of these points. The data within these groups are reasonably self-consistent, showing that on almost every separate day the same effect was produced, but that during the month's interval the subject had gotten over into a condition such that the difference between the effects produced upon him by two opposing formal suggestions was of opposite sign to what it was earlier.

Third, the mere words 'long' and 'short,' regardless of their content, seem to affect the estimation under certain circumstances. This is illustrated by Table III., which gives the sums for the mottoes containing the word 'short' and the sums for those containing the word 'long' from group *D* of both tables.

TABLE III.

Subject.	No. of Judgments.	Standard.	Mottoes Containing the Word	
			"Long."	"Short."
C.	58	1628	1702.3	1644.4
Y.	36	580	556.3	542.6

From this table it will be seen that the total for mottoes containing the word 'short' is less in both cases than that for mottoes containing the word 'long.' Similar totals from group *A* give exactly opposite results for Table I., and neutral results for Table II. (*i. e.*, in the latter case the totals are about equal).

Now these results, taken as they are merely from the group totals, may either be the result of chance, or they may be due to the existence of a different attitude towards positive and negative suggestions, causing the subject at any given time to incline to act in accordance with one and in opposition to the other; or they may be due to a tendency to be influenced by the mere words 'short' and 'long,' as said above. The likelihood of this latter explanation led to the group of experiments which follow under section 2.

Fourth, a motto which has interest for a subject seems to give greater lengths in the reproduction than an uninteresting one. The nonsense mottoes in group *A* (except for a single day for subject Y.) give smaller totals than do the other mottoes, which in this part of the experiment were not yet so familiar as

to lack interest; while in groups *C* and *D* the motto 'Life is real where,' which the subjects declared was much more empty and uninteresting than the 'nonsense' motto, and hence should be taken as the criterion in these groups, gives smaller totals than the averages for the suggestion mottoes, as show in Table IV.

TABLE IV.

Subject.	Mottoes with content. (Average of columns 7 and 9 of groups *C* *D*, omitting row 1 of *D*, Table I.)	Mottoes without content. (Total of column 5 of same days.)
C. (Table I.)	1270.8	1253.3
Y. (Table II.)	991.9	976.0

The indication of this comparison is of course very unsatisfactory, but seems at least to warrant a special investigation on this point.

2. SECOND SET OF EXPERIMENTS.

The apparent effect of the mere words 'long' and 'short' in the first set of experiments led to the second set, in which the mottoes used were only three in number, viz.: 'long,' 'short,' and 'XXXX.' The apparatus differed from that described above only in the substitution, for the pegs, of white paper squares on a black screen, one square of the pair being on a strip of black paper running in grooves behind a slot in the screen, so that the adjustments, *i. e.*, the various distances of separation of the squares, were obtained by simply sliding the strip along. Three standard distances were used, viz.: 17, 18 and 19 cm., being given in such order that each was preceded by each of the others about an equal number of times, and each of the three used an equal number of times on the same day. Each of the mottoes was given an equal number of times with each of the standards, in order determined by lot. The letters of the mottoes were so spaced as to cover the same extent in every case and so exclude the possibility of a difference due to mere space contrast or assimilation.

Four subjects were employed, and the results were not very uniform, two of the subjects showing no decided tendency towards anything resembling a constant effect, while the other

two subjects, showed a clear general constancy of considerable difference throughout. The results for these two are given in Tables V. and VI.

TABLE V.

SUBJECT K.

Date.	Standard.	Times Used.	Sum for Standard.	Sum for "Long."	Sum for "Short."	Sum for "XXXX."
Sept. 18.	17	3	51	47.8	48.4	48.3
	18	3	54	53.6	52.4	53.9
	19	3	57	54.8	56.2	58.0
			162	156.2	157.0	160.2
Sept. 24.	17	5	85	79.9	79.2	81.5
	18	5	90	84.4	84.3	85.1
	19	5	95	93.5	95.5	94.3
			270	257.8	259.0	260.9
Sept. 25.	17	6	102	94.6	98.6	98.3
	18	6	108	104.9	104.4	103.0
	19	6	114	107.5	113.6	111.7
			324	307.0	316.6	313.0
Sept. 26.	17	6	102	92.7	96.8	95.1
	18	6	108	106.6	107.2	105.0
	19	6	114	110.4	112.1	112.9
			324	309.7	316.1	313.0
Oct. 2.	17	6	102	94.1	95.5	92.7
	18	6	108	101.8	104.5	103.8
	19	6	114	111.4	114.8	113.7
			324	307.3	314.8	310.2
Oct. 4.	17	4	68	58.1	62.3	60.6
	18	4	72	67.2	65.7	68.6
	19	4	76	72.7	73.4	73.2
			216	198.0	201.4	202.4
Grand Total.		90	1620	1536.0	1564.9	1559.7

By examining Tables V. and VI. it will be seen that for Subject K. the figures for the motto 'short' are with two exceptions greater than the figures for the motto 'long,' and for subject M. the reverse is true, again with two exceptions. In the daily totals, however, there are no exceptions. In spite of the fact that the other two subjects gave neutral results, the hypothesis that the words 'long' and 'short' of themselves are capable of influencing the estimation of distances seems well

Table VI.

Subject M.

Date.	Standard.	Times Used.	Sum for Standard.	Sum for "Long."	Sum for "Short."	Sum for "XXXX."
Sept. 5.	18	6	108	108.3	103.2	108.8
	19	6	114	123.2	117.0	116.8
			222	231.5	220.2	225.6
Sept. 6.	17	7	119	118.2	113.0	118.5
	18	5	90	92.8	91.3	86.1
			209	211.0	204.3	204.6
Sept. 11.	18	8	144	144.6	140.5	141.7
	19	6	114	116.0	117.8	112.1
			258	260.6	258.3	253.8
Sept. 13.	17	8	136	139.8	134.6	137.8
	18	8	144	148.3	145.5	146.6
			280	288.1	280.1	284.4
Oct. 7.	17	7	119	117.3	116.0	119.0
	19	7	133	130.2	134.3	134.7
			252	247.5	250.3	253.7
Oct. 11.	17	7	119	121.0	115.0	117.4
	19	7	133	136.7	132.5	138.0
			252	257.7	247.5	2554
Oct. 14.	17	1	17	17.1	16.0	17.0
	18	3	54	50.3	48.9	52.9
	19	4	76	76.2	72.7	75.1
			147	143.6	137.6	145.0
Grand Total.		90	1620	1640.0	1598.3	1622.5

grounded, for in view of the discussion of the first group of experiments we might expect that the suggestion effect would be contrary in certain different subjects, and lacking in others. Thus the influence of purely formal and arbitrary suggestion seems even more clearly evidenced by this second group of experiments than by the first.

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FROM THE UNIVERSITY OF CALIFORNIA PSYCHOLOGICAL LABORATORY

COMMUNICATED BY G. M. STRATTON

Experiments on the Unreflective Ideas of Men and Women

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VIII. EXPERIMENTS ON THE UNREFLECTIVE IDEAS OF MEN AND WOMEN.

BY GENEVIEVE SAVAGE MANCHESTER.

In 1891, Professor Jastrow made a study[1] of the mental differences of men and women, using as material, lists of one hundred words each, written by men and women students in his classes. These lists were written as rapidly as possible in order that they should be natural and unreflective. From a comparison of twenty-five men's lists, with an equal number of the women's lists, he concluded that the feminine traits of mind revealed by the study are: 'An attention to immediate surroundings, to the finished product, to the ornamental, the individual, and the concrete, while the masculine preference is for the more remote, the constructive, the useful, the general, and the abstract.' A few years later, a similar experiment was made at Wellesley College. Miss Nevers,[2] who made the study, found that her results were strikingly different from those obtained by Dr. Jastrow. Later, however, it was discovered that for the most part, this difference in results was due to a deviation in method, the instruction to write the lists as rapidly as possible, having been omitted by Miss Nevers. A repetition of the experiment conforming closely to Professor Jastrow's procedure produced results which supported some of his conclusions, but not all.[3]

[1] *New Review*, Vol., V, 1891, pp. 559 to 569.

[2] PSYCHOLOGICAL REVIEW, 1895, pp. 361 to 367.

[3] Other experiments on the mental differences of men and women have been carried on, though not along the lines suggested by Professor Jastrow. Helen Bradford Thompson, in a study of the mental differences of men and women came to the following conclusions (*Psychological Norms in Men and Women*, Univ. Chic. Press, 1903, page 171): "Women are decidedly superior to men in memory, and possibly more rapid in associative thinking. Men are probably superior in ingenuity. In general information and intellectual interests there is no difference characteristic of sex." For other references and results v. Havelock Ellis' *Man and Woman* (Contemp. Science Series).

Preliminary to a further study of the mental differences of the sexes, I have repeated Dr. Jastrow's experiment at the University of California. To get the required lists, all the men and women in several classes in general psychology were given blank sheets of paper on which were spaces for 100 words, the writer's name, sex and the time required to write the list. The only instructions given were to write at top speed and to avoid writing words in sentences. From the large number of papers received, three sets were selected, each set containing twenty-five men's lists and twenty-five women's. In selecting the lists, the only requirements were that the lists should seem natural and unreflective and that the same word should not appear more than once in the same list; that is, that each list should furnish 100 different words. It is possible that this last requirement may have been a deviation from Professor Jastrow's method. Upon inquiring, he wrote me that he was not certain whether the Wisconsin lists had been kept free from repetitions or not. With a very few exceptions, the same word does not appear twice in any one paper in the California lists. Having selected the lists, the words of each set were then separately tabulated under the following twenty-five heads, the words written by the men and women being kept apart in each set: (1) animal kingdom, (2) verbs, (3) proper names, (4) adjectives, (5) implements and utensils, (6) abstract terms, (7) wearing apparel and fabrics, (8) vegetable kingdom, (9) building and building materials, (10) parts of the body, (11) geographical and landscape features, (12) other parts of speech, (13) miscellaneous, (14) interior furnishings, (15) meteorological and astronomical, (16) mineral kingdom, (17) occupations and callings, (18) conveyances, (19) stationery, (20) foods, (21) educational, (22) arts, (23) amusements, (24) mercantile terms, (25) kinship.

After classifying each of the three sets separately, they were then combined and the set thus obtained consisting of seventy-five men's lists and seventy-five women's lists, was classified as the smaller sets had been.

The division of the words under the above twenty-five heads follows the classification of Dr. Jastrow in order that the Cali-

fornia results might be comparable with those of Wisconsin, although certain objections to the division might be urged. For example it might be pointed out that the procedure of Dr. Jastrow does not admit of exact repetition, though this is essential to a correct testing of results. No two experiments would tabulate the words in the case of the twenty-five classes under exactly the same heads. This difficulty arises from the fact that some of the classes are vague. An illustration of this lack of clear definition may be drawn from the class 'interior furnishings.' Many of the household articles that women use, are of course, 'implements and utensils' and just what household articles should be classed as 'interior furnishings' and what as 'implements and utensils' is not clear. In case most of the implements women use about their work are classed as 'interior furnishings,' the preponderance of the men in the implement group loses all its significance. The class 'foods' is not clearly to be distinguished from the 'animal,' 'vegetable' and 'mineral kingdom' groups, since all foods can be classed under these three heads. Here again there is a chance for words to stray. The class 'educational' is also exceedingly vague. Each person who uses this system of classification will probably classify under this group somewhat differently. The personal factor will come in to an appreciable extent in each repetition of the experiment and absolute uniformity of method will not be secured.

These defects are easier to see than to remedy. If a classification free from them is possible, it must probably be made along more strictly logical lines. The words would need to be classified several times instead of once, selecting in each classification some one principle of division.

In two cases, I have made slight changes in Dr. Jastrow's terminology. For his term 'unique words,' *unrepeated words* has been substituted, and for the term 'different words,' *vocabulary* has been used.

In Table A the results of the California experiments are given, together with those previously reported, arranged in the order in which the experiments were performed.

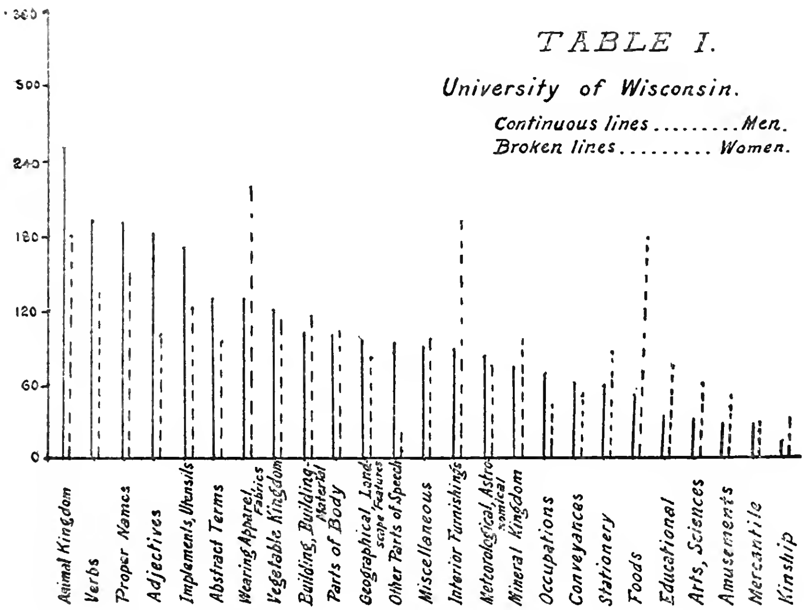
To aid in comparing the results, they have been represented

TABLE A.

	Wisconsin.		California Set I.		California Set II.		California Set III.		Welles-ley.	Combined California Set.	
	Men.	Women.	Men.	Women.	Men.	Women.	Men.	Women.	Women.	Men.	Women.
1. Animal kingdom	254	178	193	196	239	196	210	170	146	642	562
2. Abstract terms	131	97	137	117	152	114	51	71	101	340	302
3. Adjectives	177	102	207	267	201	247	216	283	300	624	797
4. Amusements	30	53	25	39	27	51	23	45	17	75	135
5. Arts	33	61	47	79	54	84	75	75	17	176	238
6. Buildings, building materials	105	117	127	131	116	145	120	144	86	363	420
7. Conveyances	62	52	40	43	42	48	51	58	19	133	149
8. Educational	34	76	68	69	59	97	50	55	102	177	221
9. Foods	53	179	104	109	85	81	53	45	88	242	235
10. Geographical, landscape features	97	80	113	104	110	116	83	123	70	306	343
11. Implements, utensils	169	121	86	45	114	99	144	103	139	344	247
12. Interior furnishings	89	190	64	82	78	128	129	147	212	271	357
13. Kinship	17	32	10	9	4	11	14	16	42	28	36
14. Mercantile terms	30	29	10	11	7	15	22	17	18	39	43
15. Meteorological, astronomical	85	76	64	53	102	99	91	110	109	257	262
16. Mineral kingdom	74	96	88	33	109	69	90	71	30	287	173
17. Miscellaneous	91	97	257	255	147	134	188	151	123	592	540
18. Occupations	71	47	47	30	58	31	76	44	24	181	105
19. Other parts of speech	96	5	112	146	90	73	123	90	164	325	309
20. Parts of the body	101	105	92	60	122	73	58	54	66	272	187
21. Proper names	194	153	98	106	99	128	56	41	81	253	275
22. Stationery	60	86	38	35	57	47	78	81	69	173	163
23. Vegetable kingdom	121	110	97	102	99	78	53	90	101	249	270
24. Verbs	197	134	286	257	259	218	360	299	279	905	774
25. Wearing apparel, fabrics	129	224	89	119	70	118	86	117	97	245	354
Totals	2500	2500	2499	2497	2500	2500	2500	2500	2500	7499	7497

graphically by a system of vertical lines, the lengths of which are proportionate to the numbers given in Table I. The continuous lines represent the number of words in each class used by the men, the broken lines, the number of words written by the women.

The order of arrangement of the classes in the diagram is, of course, arbitrary. For convenience, the classes are arranged in the order of their size, as obtained in the results of Professor Jastrow's men. The series begins with the class 'animal king-

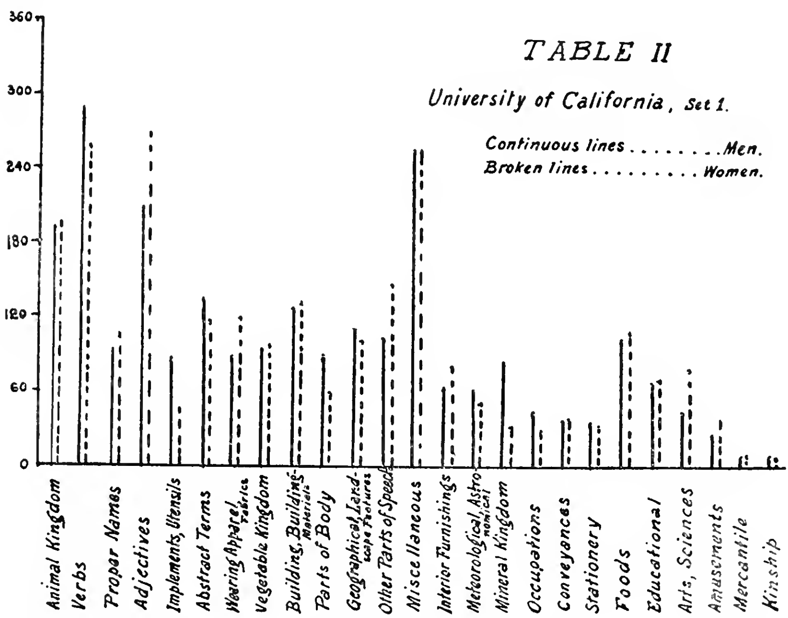


dom,' to which the men of the University of Wisconsin contributed the largest number of words, and ends with 'kinship,' the class to which they gave the smallest number of words. This order is kept for the women of the Wisconsin lists and for all the subsequent lists.

Turning now to Table I. to review the comparison of the Wisconsin men and women, it is seen at a glance that the women greatly preponderate over the men in the class of 'wearing apparel and fabrics,' 'interior furnishings' and 'foods'; to a less degree in the classes 'educational,' 'arts' and 'amusements.' In

mentioning terms denoting objects in the 'animal kingdom,' 'adjectives,' 'abstract terms' and 'implements and utensils,' the men exceed the women. Numerous other variations are shown, but these are the most striking.

A comparison of these results with those obtained by the California experiments discloses the fact that, while there is general agreement in several interesting particulars, there is nothing that can be called complete corroboration of the conclusions Professor Jastrow drew.[1] A prominent characteristic to be



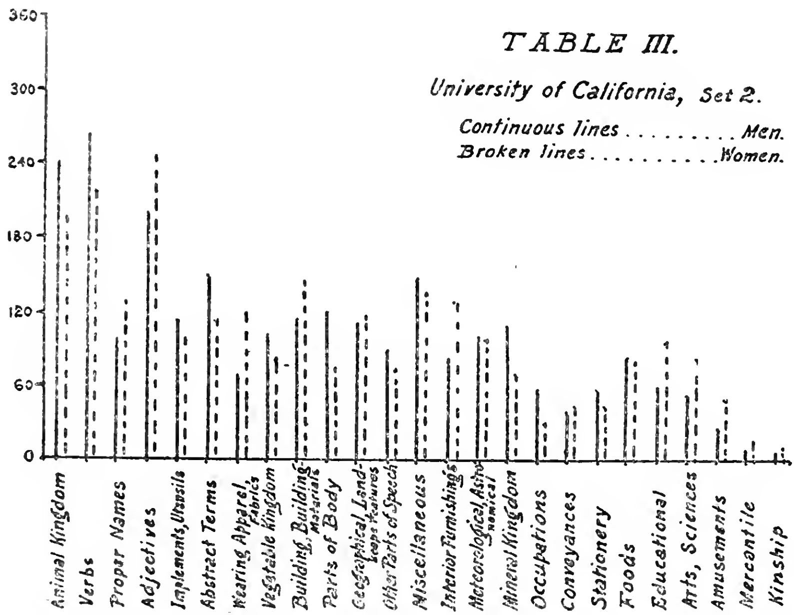
noted in the California experiments is the absence of the marked disparity between the men and women which the Wisconsin record shows. In the class 'wearing apparel and fabrics,' the California women clearly exceed the California men but in a less degree than the Wisconsin women exceed the Wisconsin men. (Compare Tables II., III., IV. and V. with Table I.)

The same relation holds for the class 'interior furnishings'; but in the case of 'foods,' one of the three classes in which the

[1] The fact that there are no men's lists in the Wellesley material is an obvious drawback in comparing them with Wisconsin and California lists and for that reason they will, for the time, be left out of account.

Wisconsin women markedly exceeded the Wisconsin men, the result differs from that obtained by Professor Jastrow. In one of the California sets, the women exceed the men slightly (see Table II.), while in the other two sets, the men are slightly in advance of the women.

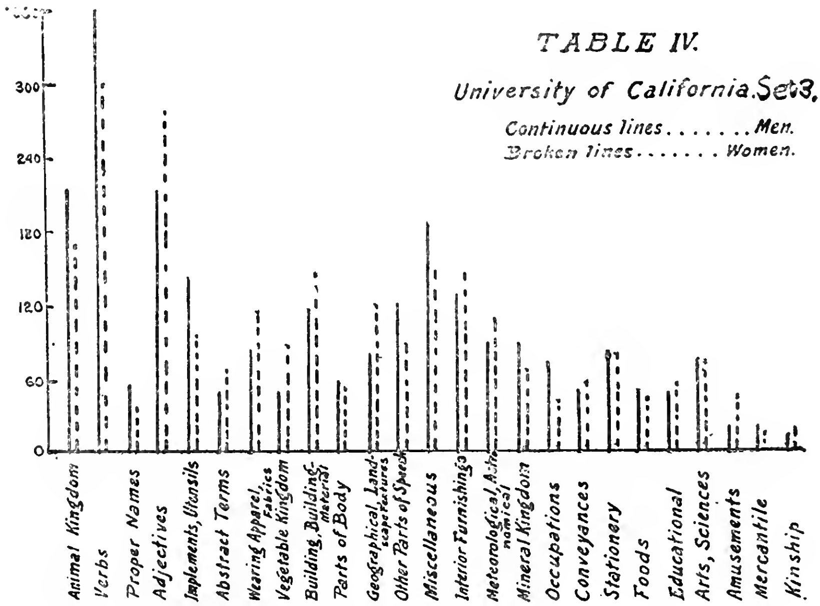
The California lists agree with the Wisconsin lists in that the women exceed the men in the classes 'educational,' 'arts,' and 'amusements'; but as before, the California men and women differ less from each other than the Wisconsin men and women.



The notable ratios in which the Wisconsin men exceeded their women classmates in terms belonging to the classes 'animal kingdom,' 'proper names' and 'adjectives' do not hold for the California lists. The California sets all agree with the Wisconsin lists in that the men lead the women in every case in mention of 'verbs' and 'implements and utensils.' A general agreement of both Wisconsin and California results is to be found in the fact that both men and women drew the larger part of their words from the classes arranged on the left half of the tables, while the classes to the right, such as 'educational,'

'amusements,' 'mercantile terms' and especially 'kinship' furnish comparatively few of the surface ideas.

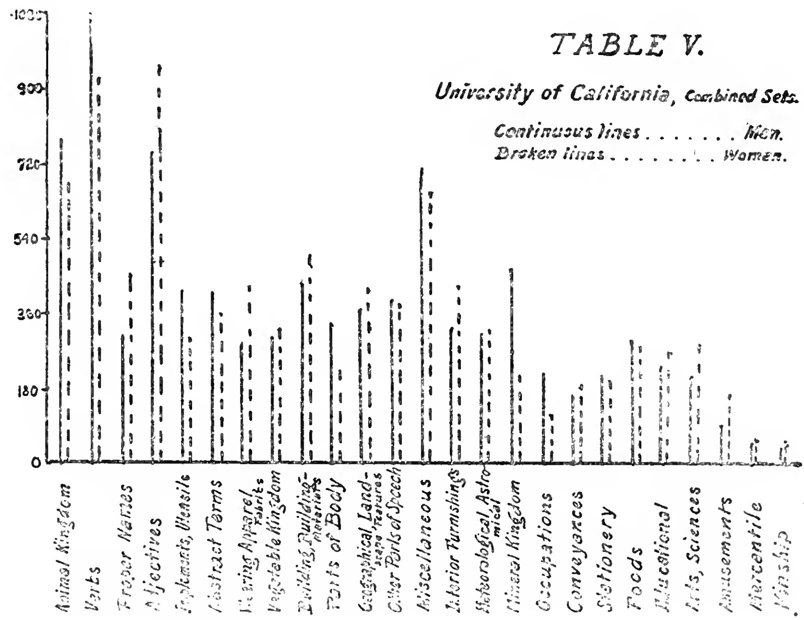
In the Wisconsin lists, Dr. Jastrow found that the vocabulary of the men was greater than that of the women. The same result was obtained in all three of the California sets. In Table VI. the results of this part of the experiment are tabulated, the results of the Wisconsin and Wellesley experiments being included. [1]



A study of this table shows that in each of the five sets, the men have used a larger percentage of different words than the women of the same set. In set II. of the California series, however, the women exceed the men of set I. in vocabulary. It is to be remarked that the women in all three of the California sets and also in the Wellesley set, exceed the Wisconsin men in vocabulary. It is also to be noted that the difference between the vocabularies of the Wisconsin men and women is considerably greater than that between the California men and women.

[1] The column headed 'percentages' indicates the ratio of the vocabulary to the total number of words written.

The vocabulary of the Wisconsin men is 10 per cent. larger than that of their women classmates, while, in no case, does the vocabulary of the California men exceed that of the California



women more than 4 per cent. In every set, both the men and women in the California experiment use larger vocabularies than either the men or women of Wisconsin. It would, of

TABLE VI.

	Unrepeated Words.		Vocabulary.		Percentages.	
	Men.	Women.	Men.	Women.	Men.	Women.
Wisconsin.	746	520	1376	1123	55	44.9
California Set I.	1000	949	1471	1397	58	55.9
California Set II.	1170	1079	1583	1509	63.32	60.36
California Set. III.	1079	978	1489	1407	59.56	56.28
Combined Calif. Set.	1975	1950	3119	3c48	41.58	40.64
Wellesley.		868		1306		52.25

course, be unwarranted to conclude from these results (though the thought is at least suggested) that the men and women of California have more diversified interests that those of Wiscon-

sin, and that the men and women of California differ from each other less than the men and women of Wisconsin. Only one set of lists is given here for the University of Wisconsin, and additional sets might change the proportions materially. The matter of the treatment of plurals may account in great part for the larger vocabularies obtained in the California experiments. I have in all cases counted the singular and plural of the same idea as two different words. The justification for this is to be found along psychological rather than etymological lines. Etymologically, the words *horse* and *horses* are practically the same, but the mental picture, or idea, corresponding to each is different. If Professor Jastrow did not make this distinction, but counted the singular and plural forms of an idea as the same word, the vocabularies in the Wisconsin study would be correspondingly low.

That the men exceed the women in vocabulary in each of the five sets, is significant, even if the superiority on the part of the men is slight. It should be remembered that in the combined California set, the total number of words was three times as large as in the other sets. It is to be noted, further, that not only the percentage of difference, but the absolute difference between the vocabularies of the men and women is greater in the 2,500-words sets than in the 7,500-word set. In the smaller sets, the difference varies from two per cent, to a little more than three per cent. while in the large set, the difference is leveled to one per cent. It seems likely that if the number of lists were indefinitely increased, the difference in vocabulary between the men and women would diminish regularly as the limits of the language were approached.

Leaving now any further comparison of the California results with those obtained by other experiments, a more profitable field for study lies in the examination of the cumulative results of the experiments at Wisconsin and California, in order to discover in what features, if any, all of the results agree. If this study is to disclose any real differences between the unreflective ideas of the sexes, such differences should be evident in all of the sets of lists, or at least in a pronounced majority of them. The most significant differences, then, revealed by these

experiments, will be those which appear in every one of the four sets.

To indicate the agreement or disagreement of the different sets as regards the classes of words used Table VII. was prepared.

TABLE VII.

+ MEN LEAD. — WOMEN LEAD.

	Wis.	Calif. I.	Calif. II.	Calif. III.	Calif. Comb.
Animal kingdom	+	—	+	+	+
Verbs.	+	+	+	+	+
Proper names.	+	—	—	+	—
Adjectives.	+	—	—	—	—
Implements and utensils	+	+	+	+	+
Abstract terms	+	+	+	—	+
Wearing apparel, fabrics	—	—	—	—	—
Vegetable kingdom	+	—	+	—	—
Buildings, building materials	—	—	—	—	
Parts of body	—	+	+	+	+
Geographical, landscape	+	+	—	—	—
Other parts of speech	+	—	+	+	+
Miscellaneous	—	+	+	+	+
Interior furnishings	—	—	—	—	—
Meteorological, astronomical	+	+	+	—	—
Mineral kingdom	—	+	+	+	+
Occupations	+	+	+	+	+
Conveyances.	+	—	—	—	—
Stationery.	—	+	+	—	+
Foods.	—	—	+	+	+
Educational.	—	—	—	—	—
Arts, sciences	—	—	—	—	—
Amusements	—	—	—	—	—
Mercantile terms.	+	—	—	+	—
Kinship.	—	+	—	—	—

In this table, the classes in which the men excelled the women in the number of words written are marked by a plus sign, while the classes in which the women led are indicated by a minus sign.

A review of the results of the classification into the twenty-five groups, shows a rather remarkable agreement in the five sets tabulated in Table VII. Of the twenty-five classes, eighteen show an agreement, either in all five cases or in four cases out of five. In the three classes in which the men lead most pronouncedly, namely, in 'verbs,' 'implements and utensils' and 'occupations,' the notion of action is prominent. In the classes in which the women lead, such as 'adjectives,' 'wearing apparel and fabrics,' 'interior furnishings' and 'buildings

and parts of buildings,' no such notion is evident. On the contrary, these show a preference for things at rest.

Running through all, or nearly all, of these agreements in results, there appears, beside the notion of action as contrasted with inaction, another element. In the cases in which the men lead, time is an essential factor; in the cases in which the women lead, space is the more prominent consideration. While the time and space conceptions are closely allied to the notions of action and inaction, they seem, nevertheless, to be not altogether identical. A further inference may be drawn from the cases in which there is agreement in all five sets. The fact that the men throughout draw a large percentage of their words from such classes as 'implements and utensils,' 'occupations,' and 'verbs' than the women do, and from the fact that the women lead throughout in the classes 'wearing apparel and fabrics,' 'buildings and building materials,' and 'interior furnishings,' it may be inferred that the unreflective ideas of both men and women concern the objects with which they are familiar and in which they have considerable interest. That the classes 'implements and utensils' and 'occupations' are of special masculine interest, will probably not be questioned, but such an interpretation of the verb class needs justification. This justification is found in the character of the verbs used. In a very large percentage of the cases in which men use verbs, these verbs are suggestive of action in the field of men's especial interest.

To make this clear, two lists of verbs are submitted, one secured from three men's papers and the other from three women's, the lists being taken at random in both cases. Such words as 'address' and 'telegraph,' classed here as verbs, may also be nouns. In such cases the context was taken as a guide in determining the sense in which the writer used the word. The lists follow:

A. (1) address, (2) telegraph, (3) fall, (4) rise, (5) call, (6) forget, (7) ride, (8) play, (9) ringing, (10) reading, (11) studying, (12) work, (13) play, (14) tick.

B. (1) shoot, (2) tick, (3) twitch, (4) cure, (5) hit, (6) miss, (7) aim, (8) blow, (9) shoot, (10) sail, (11) hoist, (12) lift, (13) pump, (14) rush, (15) study, (16) judge.

The first series of verbs was taken from women's lists, the second from men's. The masculine character of the second series is unmistakable and was so judged by several persons to whom the lists were read without disclosing the actual source of each group.

With this further knowledge of the verbs used, it seems safe to say that the evidence justifies the statement that the unreflective ideas of men are controlled by the familiar and interesting acts and objects of their lives. Similiarly, the classes 'wearing apparel and fabrics,' and 'interior furnishings,' are recognized categories of peculiar feminine interest. It might be suggested, however, that buildings and building materials are not distinctly familiar and interesting to women. The objection falls when the character of the words drawn from this class is known. The words used were names of particular buildings and parts of houses, such as court house, church, gate, door, floor, fence, steps, marble. There is scant mention by the women of such distinctive building materials as bricks, mortar, cement and stone.

Just why women should exceed men in the classes 'arts,' and 'educational' is not evident, but taking the cue from the former cases, it seems that it might be because these classes of objects are more familiar to women than to men and of relatively more importance to them. Though men are the chief creators of art, women are more familiar with the ordinary art products, such as pictures and musical compositions. Similarly in educational matters, the women were probably more impressed with the parts of the educational system, such as lectures, texts and examinations, owing, perhaps to the fact that many of them were preparing to become teachers.

That the women lead in the category of amusements, is probably due to the fact that women, as a class, have more leisure than men. With the time to enjoy amusements of various kinds, women indulge more in them. Hence these things are more familiar to women than to men and come more readily to mind when there is a call for a rapid gathering of ideas.

Leaving now the cases where there is an agreement throughout all five sets, the agreements which appear in four

sets out of five deserve a word. In the classes 'animal kingdom,' 'abstract terms,' 'parts of the body,' 'miscellaneous,' 'mineral kingdom' and 'other parts of speech,' the men exceed the women in four sets out of five. The mention of objects belonging to the 'animal kingdom' is plentiful in the lists of both men and women, the difference between the number of times such words are written by men and women being, on the whole, not very large, but the fact is notable that in all cases, but one, the sex naturally most familiar with the various members of the 'animal kingdom' is the one which leads in this class. In the class 'abstract terms,' a similar condition prevails. While the total numerical lead of the men over the women is not great, it is, nevertheless, to be found in four cases out of five. That men have more interest and training in mathematical, physical and philosophical abstractions, might account for this difference. The significance of the men's lead in mention of 'other parts of speech' is not clear. In the classification used, this term embraces conjunctions, prepositions, interjections, pronouns and adverbs. Since the class was not subdivided, it is impossible to tell in which of the four parts of speech the men markedly excel. As they lead in verbs, it might be expected that they would lead in adverbs. So too, it might be expected that men would lead in prepositions and conjunctions since these deal with abstract relations.

Men also lead the women four times out of five in mention of 'parts of the body.' Just what, if anything, this implies, I have not determined, though it may be that the greater importance of the parts of a man's body, as tools for his daily use, keeps the conceptions of them more constantly in his mind.

That men lead in the class 'miscellaneous' may mean any one of several things. If the other groups in the classification happened to be more adapted to catch the surface ideas of women than those of men, the 'miscellaneous' class would be correspondingly large for the men. Another explanation of the men's superiority here might lie in the fact that men are interested in a greater number of objects and activities.

The lead of the men in the 'mineral kingdom' is probably due to the fact that they carve and chisel, mine and build, while women ordinarily do not.

'Conveyances' might have been discussed under the head of 'amusements,' as most of the words in this class seemed to be of that general character.

That the women in all of the California sets lead in the mention of adjectives seems significant. It has been found in the classes discussed before that the surface ideas of men concern action, the tools used in the performing acts, and the differentiations of particular acts into occupations; that is, their ideas are related to construction. The women, on the other hand, excel the men in the mention of articles of dress, house fittings, parts of houses, particular buildings and art products. There seems to be a real principle of difference here in the general character of the words written by the men and those written by the women. Men speak of the process of creating, women of the thing created. This is the same conclusion reached by Dr. Jastrow.

In view, then, of this tendency of the feminine mind toward things, rather than the doing of things, it seems natural that women should be more familiar with the qualities of things than men are. This preponderance of adjectives in the women's lists seems to bear out this supposition.

In the mention of terms of kinship, it is of interest to note that both the men and the women draw very sparingly from this class. This fact seems rather in opposition to the hypothesis before advanced that the surface ideas are of things familiar and interesting, for of course one's kindred are usually the persons seen oftenest and considered of greatest importance. It is probable that unconscious or even conscious selection played a part here. It is very likely that the persons writing the lists considered this work a sort of official act since the papers were to be returned to an instructor, and they would naturally refrain to a certain extent, from speaking of parents and other relatives. The very attitude the mind took in performing the task would inhibit ideas drawn from the family life.

In the classes which have not yet been discussed, 'proper names,' 'meteorological and astronomical terms,' 'stationery,' 'foods' and 'mercantile terms,' there is not enough regularity to warrant the drawing of any conclusions. In the class 'proper names,' it would be expected that the women would lead con-

siderably, since they show a preference for the concrete, rather than the abstract, but in two sets out of five, the men are ahead. Again, the natural expectation would be that men would write many more astronomical and meteorological terms than women, but in this class the men lead in only three cases out of five. Oddly enough, the men on the whole, in the California sets, lead slightly in foods, and the women in mercantile terms, very different results from those obtained by Dr. Jastrow.

Coming now to the last feature to be examined, namely, the time required to write the lists, it is seen from Table VIII. that the average time for the women of California is 5 minutes and 39 seconds. The average time for the men of California is 5 minutes and 47 seconds.

TABLE VIII.

	Average for Men.	Average for Women.	Average for Men and Women.
Wisconsin.			5 min. 8 sec.
California I.	6 min. 2 sec.	6 min.	6 min. 1 sec.
California II.	6 min. 17 sec.	5 min. 55 sec.	6 min. 6 sec.
California III.	5 min. 2½ sec.	5 min. 2 sec.	5 min. 2 sec.
Wellesley		5 min.	
Combined Calif. ...	5 min. 47 sec.	5 min. 39 sec.	

As Professor Jastrow does not give the average time for each sex separately, and as there are no men in the Wellesley list, these sets are not available for this comparison.

In each of the California sets, the time the women required to write the lists is slightly less than that of the men. This may mean, either that the women associate more quickly, or that they write more rapidly. That each student kept his own time renders these results less trustworthy than they would otherwise have been.

Having now presented the evidence at hand, it only remains to review the differences in the unreflective ideas of the sexes as suggested by these experiments.

From the results of the classification into the twenty-five groups, the following general statements in regard to the surface ideas of men and women may be made:

1. The surface ideas of both men and women pertain to objects which are familiar and interesting.

2. The dynamic aspect of objects is more attractive to men, while the static or completed aspect appeals more to women.

3. Time as a factor enters more largely into the surface ideas of men; space is more often a prominent feature of the surface ideas of women.

4. Men make a greater use of abstract terms, while women show a preference for the concrete and for descriptive words.

From the tabulation of the words used into unrepeated words and vocabulary, it is found that the range of the surface ideas of men, as a group, is slightly greater than that of women.

From the examination into the average time required for writing the lists, it is found that women are able to write one hundred associated surface ideas in somewhat less time than men.

All of the specific tendencies above mentioned seem to give concurrence to a general principle of difference between the sexes. The surface ideas of men are extensive rather than intensive, while the opposite is true of women. This conclusion is supported, not only by the fact that men show a preference for abstract terms, for action and for the time idea, while women prefer the concrete, the completed object and space relations, but also by the facts that men have a greater range of surface ideas than women, and to a certain extent by the fact that women have their reflective ideas more at hand, as shown by their shorter list-time. Men are interested in far-reaching relations existing between things; women give more attention to the minute analysis of things themselves.[1]

[1] The MSS. of these Studies were received October 17, 1904. — ED.

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[IX]

THE DIFFERENCE BETWEEN MEN AND WOMEN IN THE RECOGNITION OF COLOR AND THE PERCEPTION OF SOUND

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University of California.

(Communicated by Professor Howison.)

I. THE RECOGNITION OF COLOR.

In testing the sensitivity of the eye to colors, Dr. Nichols found that men where more sensitive to red, yellow and green, while the women excelled in blue. He mixed a white powder with colored pigments. A series of mixtures, varying from white to clearly colored mixtures, were presented to the subjects (31 men and 23 women), who sorted them according to shade and hue. When once the women recognized the color of the compound, they were more accurate in detecting the degree of saturation of the various mixtures; yet with the exception of blue the women required more parts of the pigment mixed with the white before the tint of the compound was recognized.

Miss Thompson, on the other hand, finds that women are the more sensitive to colors. She tested 20 men and 20 woman with five colors, and found that the women could recognize squares of colored paper (red, green and blue) at a greater distance than could the men. The squares were pasted on cards, one black and one white. The tests were conducted in a dark room, the cards being illuminated by a Welsbach burner. Her conclusion is that men's eyes are surely less keen in the recog-

[1] Edward L. Nichols, 'On the Sensitiveness of the Eye to Colors of a Low Degree of Saturation,' *Am. Jour. of Science*, Series 3, Vol. 30 (1885), p. 37.

nition of the color of an object. "Yellow is the only color for which the men's record is better than the women's." [1]

By a different method than either of the two mentioned above, I attempted to discover the difference between men and women in the recognition of color. In the following experiments a Glan spectrophotometer was used; the source of light was a Welsbach burner placed .43 meters from the instrument.

Before beginning my work, August, 1903, I selected five bands of color in the spectrum, which appeared to me to be characteristic of red, yellow, green, blue and violet. The wavelengths of the middle of the five bands were approximately, λ 6575, λ 5800, λ 5250, λ 4700 and λ 4450, respectively. The five bands were spacially equal in width, and measured about one twentieth of the length of the visible spectrum. During the tests all the spectrum was cut off except the band which stood for one of these five colors. The subject then looked into the eye-piece of the spectrophotometer, and was asked to observe and name the color, while the intensity of light was raised from 0° to a maximum, by slowly revolving the Nicol prism of the apparatus through an angle of 90°. The figures in tables I., II., and III., represent the readings in this angular scale, and not the absolute intensity of the light. The intensity of the light at any time varies approximately as the $\sin^2 a$; a being the reading on the scale.

Yet even at 0° there is some illumination of the spectrum (due to dispersion of light by the prism), so that at 0° every subject was able to see a band of light, which in general appeared white or gray. A few subjects could confidently recognize the color of the band at 0°. The violet end of the spectrum is of such low intensity that a blue-violet was chosen. The yellow strip selected includes all the yellow in the spectrum with a narrow band of yellow-green on one side, and a narrow band of orange on the other. A strip narrow enough to have included only pure yellow would have been too narrow to be used with the other colors, without being recognized by its lessened width, and, on the other hand, the width of the other colors could not

[1] Helen Bradford Thompson, *Psychological Norms in Men and Women*, University of Chicago Press (1903), p. 87.

well be reduced without making their area so small as to be recognized with difficulty by untrained observers.

Three separate hours were given to the experiment by each of the 40 subjects. During the first hour the subject was tested with Holmgren's worsteds for color-blindness. This took from 30 to 40 minutes. The room was then darkened, and the remainder of the hour was spent in practice upon observation of strips of the spectrum. The second hour the subject rested his eyes for 20 minutes in the darkened room before looking into the photometer. One determination was then made for each of the 5 colors, but with one eye only. The third hour was a repetition of the second, except that the other eye was used.

All conditions were kept as uniform as possible; the pressure of the gas was regulated by an automatic machanism and constantly noted; and the intensity of the color was increased at a constant rate of 1° per second. At the end of every 10 seconds, the subject looked away and rested his eye. Ten men and ten women used the right eye first, and an equal number of men and women used the left first. The order in which the colors were presented was varied; a given order was presented to but one man and one woman. The subjects were not told what colors would be shown them, nor that the same color would be seen for a second time. They were asked to look into the instrument, to report as soon as they saw any color, and to name the color as soon as they could recognize it.

There is a disadvantage in depending entirely on the name given to the color seen, but this was minimized as far as possible, for any peculiarity in naming the colors was noted when the subjects sorted the Holmgren worsteds; and when there was any doubt in my mind, or in the mind of the subject, the worsteds were brought out again and correction was made for any peculiarity in nomenclature. At times I could decide on no threshold (as in a case when the subject saw no green at all, but at the maximum intensity called that color yellow). It therefore happens that the average and median are at times calculated on less than 20 cases. I did not assume that the threshold for such subjects, as the one just mentioned, was greater than 90, for at 90 the strip was distinctly visible to him, and any increase in intensity

would only serve to make it more yellow. Even when green was suggested to him, he refused to accept it as a proper name for the color.

The results of these tests, according to one method of computation, appear in Table I. Using the average as the basis of our deductions, it would appear that the right eye of men is better than the right eye of women for all colors but violet; and

Table I.

Thresholds for the Recognition of Color (in Angles of Nicol's Prism).

	Right Eye.					Left Eye.				
	Red.	Yellow.	Green.	Blue.	Violet.	Red.	Yellow.	Green.	Blue.	Violet.
Women.										
Average....	11.6	13.8	11.3	25.0[2]	*24.3*[1]	8.8	*7.9*	8.0	28.8[2]	26.4
M. V. from Av.	8.4	9.5	5.1	11.2	8.0	3.7	5.6	2.5	15.3	9.7
Median ...	*7.5*	10.0	10.0	20.0[2]	*20.0*	10.0	*7.0*	8.0	24.0[2]	25.5
Men.										
Average....	*8.9*	*11.5*[2]	*8.0*[3]	*16.9*[2]	28.4[3]	*7.6*	13.7	*7.3*[3]	*17.4*[2]	26.6[2]
M. V. from Av.	3.6	6.7	2.0	7.3	11.5	5.1	8.3	2.4	10.8	10.4
Median ...	8.0	*8.0*[2]	10.0[3]	*14.0*[2]	25.0[3]	*6.5*	10.0	*7.0*[3]	*14.5*[2]	*19.0*[2]

Table II.

	Right Eye.					Left Eye.				
	Red.	Yellow.	Green.	Blue.	Violet.	Red.	Yellow.	Green.	Blue.	Violet.
Women.										
Average....	8.4	*5.8*	9.9	17.5	*22.4*	7.5	*4.8*	*7.2*	18.8	25.1
M. V. from Av.	6.4	3.7	3.2	6.6	5.9	2.1	3.8	2.9	9.0	8.8
Median ...	7.0	*4.5*	10.0	18.0	*20.0*	7.5	*4.0*	8.0	18.0	25.5
Men.										
Average....	*7.7*	8.2	*9.4*[2]	*15.0*[2]	27.6[3]	*6.3*	7.0	12.0	*14.3*[2]	*23.3*[3]
M. V. from Av.	4.6	3.4	3.2	7.0	11.0	4.6	3.6	8.5	2.5	6.8
Median ...	7.0	7.5	*9.0*[2]	*14.0*[2]	23.0[3]	*5.0*	9.0	*7.5*	*14.0*[2]	*20.5*[3]

[1] An italic numeral in tables I., II., and III., indicates that it is less than the corresponding numeral for the other sex.

[2] 19 subjects.

[3] 17 subjects.

Table III.

	Right Eye.					Left Eye.				
	Red.	Yellow.	Green.	Blue.	Violet.	Red.	Yellow.	Green.	Blue.	Violet.
Women.										
Average.. . .	*5.1*	*3.8*	*7.0*	12.0	17.0	5.9	*3.4*	*6.4*	14.6	21.1
M. V. from Av.	4.1	3.4	3.5	7.2	8.3	3.4	3.2	2.8	8.1	9.2
Median . . .	*3.5*	*4.0*	*7.0*	13.0	19.0	5.0	3.0	*5.0*	14.0	23.0
Men.										
Average.. . .	6.5	4.6	7.5	*10.4*	*14.7*	*4.9*	3.6	6.8	*9.5*	*13.8*
M. V. from Av.	5.0	3.7	3.7	3.9	9.1	4.8	3.3	3.3	4.8	8.7
Median . . .	6.0	4.5	8.0	*10.0*	*17.0*	*2.5*	3.0	7.5	*10.0*	*17.5*

that the left eye of men is better than the left eye of women for red, green and blue; equal for violet, and worse for yellow. The norm would lead to the same conclusion, but if the median is used in place of the average, a somewhat different result is obtained. When we consider the large variation, we are probably not justified in saying more than that women are in general less keen in the recognition of colors. Yellow and violet are the only colors in which they seem to excel, and in these they excel with but one eye.

Yellow was the color most difficult to name at a low intensity, for the traces of green and orange on the edges of the strip selected confused the subjects. Only a few saw a uniform color. More men than women detected the presence of green and orange. The large threshold of both men and women is probably due to the inability to find an appropriate name for this complex, rather than to a low sensitivity to the color itself. The larger threshold of the men may possibly be due to their greater sensitivity to the green and orange, and consequently their greater confusion.

The women's failure to name the colors correctly at a low intensity cannot be due to their ignorance of the proper name, since at the maximum intensity they named the colors as accurately as did the men. Table IV. gives the different names applied to the colors as they appeared at the maximum intensity, with the number of men and women using each name. The parenthesis indicates that the subjects added those words after

being pressed for a closer description, and in many cases only after other colors had been suggested to them. In some cases the subjects gave two and even three names to the strip of color shown them. As the strips were practically uniform, with the one exception of yellow, it is probable that the knowledge that a

TABLE IV.

NAMES GIVEN TO THE STANDARD COLORS.

	Right Eye.		Left Eye.	
	Women.	Men.	Women.	Men.
Red.				
Red	14	12	17	11
Red (towards orange)	4	4		3
Red (towards purple)	2	2	1	2
Orange-red		1	2	4
Red-orange		1		
Yellow.				
Red, yellow and green	8	11	5	9
Orange-red and yellow	6	6	10	6
Red and orange	5		3	2
Orange		1	1	
Orange-yellow	1	1	1	3
Yellow		1		
Green.				
Green	8	5	3	3
Green (towards yellow)	3	4	6	3
Yellow-green	7	6	10	8
Yellow and green	1	2		2
Yellow, green and blue	1		1	
Orange and yellow-green				2
Yellow		2		
Green-yellow		1		2
Blue.				
Blue	7	8	6	7
Violet-blue		4	5	2
Green-blue	6	3	5	7
Violet, green and blue	6	3	3	1
Blue-green		1		1
Blue-violet	1	1	1	1
Blue and violet				1
Violet.				
Violet	12	14	14	5
Violet (towards blue)				3
Violet (towards red)				1
Blue-violet	6	2	6	8
Violet-blue	2	1		1
Blue		2		2
Blue (towards green)		1		

spectrum was being used helped to suggest the neighboring colors, though no doubt some subjects did actually discern a difference in the two edges of the band.

At the close of the tests, 10 men and an equal number of women were shown the whole spectrum, and asked to select the 5 portions which they called the best and purest bands of red, yellow, green, blue and violet. The results are given in Table V. The numbers represent the centers of the narrow

TABLE V.

READINGS IN CASE OF FREE SELECTION OF COLORS.

Women.

	Right Eye.					Left Eye.				
	Red.	Yellow.	Green.	Blue.	Violet.	Red.	Yellow.	Green.	Blue.	Violet.
Average	8.42	10.57	12.99	15.96	17.93	8.53	10.84	13.13	16.18	17.92
M. V. from Av .	.17	.19	.45	.27	.28	.18	.31	.34	.25	.24
Median	8.45	10.48	12.75	15.82	18.01	8.52	10.83	13.25	16.20	17.92

Men.

Average	8.47	10.83	12.57	16.13	18.23	8.52	10.74	12.89	15.99	18.34
M. V. from Av .	.09	.21	.43	.60	.40	.14	.38	.41	.25	.55
Median	8.48	10.80	12.88	15.94	18.24	8.51	10.75	12.90	16.03	18.30
Readings actually used for standard colors	8.50	10.50	12.50	16.00	18.00					

band chosen, the visible spectrum running from about 7 to 22 — beyond 23 for some of the subjects. The standard colors used in the preceding tests are represented by the numbers 8.5, 10.5, 12.5, 16 and 18. The colors selected by the subjects are close to those used in the tests. The wave-lengths are approximately the same except for yellow and green. λ 5700 was chosen for yellow by the left eye of women, and by the right eye of men; and λ 5150 was chosen for green by the left eye of women. This is consistent with the fact that so large a number called the standard green a yellow-green (see Table IV.). From the names given to the standard violet we might expect the subjects to choose a violet farther away from the blue. It is true that the men did, yet the difference between 18 and 18.34 is not perceptible. In endeavoring to choose the colors on dif-

ferent days, I find in my own case that the variation from the average is between 1 per cent. and 1.5 per cent. for red, yellow, green and violet, and a little over 2 per cent. for blue, being but little less than the variation from the average of the 10 women.

Instead of waiting until the subject could name the color with certainty, we might take, as the more probable threshold of recognition, the reading where the color was first correctly named, even though it was named with doubt and reservation, and also the reading where the subjects gave a name so near the standard as to make it probable that they recognized the standard color, but had not secured the most appropriate name. For comparison I have calculated this probable threshold and in so doing I have allowed myself some freedom. I have not taken the lowest reading when my knowledge of the subject and the evidence of later reports made it certain that the first correct report was merely a lucky guess.

Comparing these results, given in Table II., with those given in Table I., we see that woman's record with the right eye is here lower than man's for yellow as well as for violet; her record with the left eye is still lower for yellow, and in addition is probably lower for green. These results, differing as they do from those obtained from Table I., may simply mean that the men were more cautious, that they did not venture a name until they were fairly sure it would fit; while the women would speak as soon as they discovered the region to which the color belonged, afterwards locating it more exactly.

Going over the original data for a third time, I calculated the thresholds in still another way. The threshold is now taken to be the reading where some closely kindred color or some component of the color, as it appeared to the subject at the maximum intensity, was named. For example, if at the maximum a color appeared green-blue to a certain subject, then we may assume that at the first mention of either blue or green, the color was really perceived by that subject. Table III. shows the thresholds calculated in this way. Orange was accepted as a correct answer for either red or yellow; if the green seemed a yellow-green, then either yellow or green was accepted for

green. Ample allowance is thus made for individual differences of nomenclature; and we have a threshold of recognition of something, which is a close approach to the standard, if it is not the standard itself. Woman's right eye is superior to man's right in this kind of recognition when the standards are red, yellow and green; her left is superior to man's left when the standards are yellow and green. The superiority of men in blue and violet is, on the whole, more marked than is the superiority of women in yellow and green.

When we compare Tables I., II. and III., we find that in spite of some discrepancies, there is a certain uniformity. In every instance the men have a lower threshold for blue. For the left eye they have a practically equal or lower threshold for violet; for the right eye a greater for violet, except when blue or violet-blue is accepted as a correct answer. It seems certain that the men could distinguish blue and the blue factor in the violet at a lower intensity than could the women. In the blue of the 3 tables there is at no time more than 4 men who fail to excel the average woman with the left eye; and never more than 7 men who fail to excel the right eye of the average woman. With the exception of the right eye in Table I., and possibly the left in Table III., women have the lower threshold for yellow; yet there are always from 6 to 10 women who fail to excel the average man. Women excelled in red when orange was accepted as a correct answer. The instances when they excelled in green are those in which yellow and green-yellow are accepted as correct answers.

Taking into account all 3 methods of calculating the thresholds, the general conclusion would be, that men are clearly superior in the recognition of blue; and women are possibly superior in the recognition of yellow. These results do not agree at all with those of Miss Thompson, who found that men excelled in yellow alone. The difference between my results and those of Miss Thompson may be due to the fact that 14 out of the 25 men she tested fell into the two classes, which she designates as 'color-blind' and 'poor in color discrimination'; while none of her women were 'color-blind,' and only 4 were 'poor.'[1] No color-blind person is included in my report,

[1] *Psychological Norms*, page 88.

although 5 men and 3 women might be called poor in color discrimination. Their mistakes consisted in mixing some of the blues and greens, and in failing to detect anything but a pale tint of red in our laboratory sample of Holmgren's purple. In naming the spectrum colors, however, these subjects appear to be as good as the average subject tested.

Miss Thompson finds that the left eye of woman excels her right in all but yellow. In my tables her left eye seems superior in yellow and green; yet when the number of individuals are counted up, there are but 11 whose left eye excels the right in yellow, and 14 in green; for the other three colors there is no difference between the two eyes. Miss Thompson finds man's left eye has a better record in yellow alone. I find that in yellow there are 12 men who have a better right eye, against 6 who have a better left; yellow is the only color in which his right eye excels. In red and green I find no difference, but in blue and violet the left eye excels; in blue 5 men have a better right eye, 8 a better left; in violet 9 have a better left, 3 a better right.

Each of the subjects was asked which eye he preferred to use. Most of them had no preference. Of the 20 women, 5 prefered to use the left eye. Each gave, independently, the same reason; in looking into the instrument with one eye they alway kept the other closed, and could with less effort keep the right eye closed. One, a left-handed woman, prefered to use her right for the same reason, that is, she found it easier to keep her left eye closed. Only 2 men expressed a preference; both preferred to use the right, they were in the habit of looking into instruments and were accustomed to using the right eye. In the results of these subjects I can find no consistent advantage of the preferred eye over the other.

II. The Perception of Sound.

The following experiments were undertaken with the object of finding any difference that may exist between men and women with regard to the absolute threshold for hearing. The individuals tested, 20 men and 20 women, were all students in the University of California.

An electric tuning-fork of 100 double vibrations was used in these tests. The noise which attends the sparking of the electric contact was avoided by connecting up the fork in series with a second fork of 100 vibrations in a distant room; this second fork interrupted the current in the first and permitted its use with continuous closed contact, and thus a noiseless pure tone was obtained. By means of a galvanometer and resistance the current was kept constant; and by the constant use of commutators any permanent alteration in the magnet of the tuning-fork was prevented. The subject sat in an ordinary research room, behind a screen in which was an aperture large enough to receive the ear. During the tests the subject sat with his head pressed against the screen, his ear within the aperture. The intensity of the sound was altered by varying the distance of the tuning-fork from the screen.

For each of the subjects, a first rough estimate of the greatest distance at which the sound could be heard was made. Then beginning somewhat within this distance, tests were made at intervals of 2.5 centimeters. At each interval a group of 10 tests were made, 5 with the tuning-fork going and 5 with it stopped. The order in which the two kinds of tests were taken was constantly varied. All the series of groups were run from above to below the threshold. The subject was given a warning; he then placed his ear at the aperture and reported whether he heard, or did not hear the sound. When the subject was doubtful the test was repeated. Between the groups the subject relaxed his position and rested. The greatest distance at which 80 per cent. of the answers were correct was recorded as the threshold, though groups of tests were also made beyond this point to make sure that the falling off of correct answers was not accidental nor momentary. The thresholds are recorded in Table I.; the figures represent the number of centimeters from the screen. The average and also the median of the 20 subjects are given. One man's threshold was more than twice as great as that of any other one of the 40 subjects. I have therefore also given the average of 19 men, excluding this one. Among the women there was none that seemed exceptional.

TABLE I.

THRESHOLD FOR HEARING IN CENTIMETERS. SERIES I.

	20 Women.		20 Men.	
	Right ear.	Left ear.	Right ear.	Left ear.
Average	44.75	38.00	70.25	53.50
Median	42.50	35.00	53.75	48.75
			19 Men.	
		Average . . .	54.50	48.50

VARIATION FROM THE AVERAGE.

20 Women.		20 Men.	
Right ear.	Left ear.	Right ear.	Left ear.
9.8	9.1	21.0	12.8
		19 Men.	
		8.7	7.7

The average women of the 20 could hear 17 per cent. farther with the right ear than with the left. The average man of the 20 could hear 31 per cent. farther with the right than with his left. The average man of the 19 could hear 12 per cent. farther with his right than with his left.

The average man of the 20 could hear 19 per cent. farther with his left ear than the average woman could hear with her right ear. The average man of the 19 could hear 8 per cent. farther with the left ear than the average of the 20 women could hear with her right.

The results show that both the women and the men could hear farther with the right ear than with the left. The men hear much better than the women. There was but one woman who excelled the average man; and, on the other hand, only 3 men fall below the average women. Eight women fall below the lowest man. The men not only could hear further than the women, but the poorer ear of the men was much keener than the better ear of the women.

A second set of experiments was then undertaken, this time with a tuning-fork of 500 double vibrations. The same subjects, with the exception of 2 men and 2 women were used in these tests. More precautions against external noises were taken. The subject sat in a silent room, from which all noises were excluded by specially constructed walls and doors. One end of a lead pipe has its termination in this silent room, while the

other end terminates in a research room in another wing of the laboratory. The screen used in the first series of tests was set up in the silent room, 1.675 meters from the end of the lead pipe. The tuning-fork of 500 vibrations was set at varying distances from the other end of the pipe in the distant room where the experimenter was. The appearance of a light in the dark room served to warn the subject, who then, as before, placed his ear at the aperture in the screen. Here and in the earlier series of experiments, half of the men and half of the women used the right ear first, the other half used the left first. The subject communicated his answers by means of a telegraphic key. After a first rough estimate of the threshold was made, 10 tests were made at regular intervals of 1 centimeter. The method used was the same as the first group of tests. The figures in Table II. are in centimeters, and represent the distance of the tuning-fork from the end of the tube.

TABLE II.

THRESHOLD FOR HEARING IN CENTIMETERS. SERIES II.

	18 Women.		18 Men.	
	Right Ear.	Left Ear.	Right Ear.	Left Ear.
Average.	6.88	5.66	8.61	7.77
Median	7.00	5.50	8.50	6.00

VARIATION FROM THE AVERAGE.

20 Women.		20 Men.	
Right Ear.	Left Ear.	Right Ear.	Left Ear.
3.50	3.16	5.00	4.80

The average woman could hear 21 per cent. farther with her right ear than with her left ear. The average man could hear 10 per cent. farther with his right than with his left ear.

The average man could hear 11 per cent. farther with his left ear than the average woman could hear with her right ear.

The results are as before: the men hear farther than the women; the right ear of both men and women is keener than the left. The changed conditions and the difference in the note has not changed the general result, though it affected the relative positions of individuals within the groups of subjects. The two men, whose records were the highest in the tests with the tuning-fork of 100 vibrations, fell below the average of the men

when the fork of 500 vibrations was used. The two women who stood highest in the first series of tests also stood highest in the second series; but in general it is not true that the women who heard farthest in the first series, also heard farthest in the second series. Fully 50 per cent. of the men whose records were higher than the median in the first series, fell below the median in the second series; the same is true of the women, although the women do not make such a decided change. They change from a short distance on one side of the median to a short distance on the other. The superiority of the men is not so marked. With the left ear, 7 women excel the average man; with the right ear, 4 women excel the average man. Eight men fall below the average woman with both the right and left ear.

The superiority of the right ear over the left is as marked in this second series as it was in the first. Not only was the right ear of the average man and average woman better than the left, but the right ear of almost every individual subject was either better than, or equal to, the left (see Table III.). Of the 40

SERIES I.

Number of women with a better right ear	16
Number of men with a better right ear	14
Number of women with a better left ear	1
Number of men with a better left ear	2
Number of women with the right and left ear equal	3
Number of men with the right and left ear equal	4

SERIES II.

Number of women with a better right ear	12
Number of men with a better right ear	14
Number of women with a better left ear	3
Number of men with a better left ear	3
Number of women with the right and left ear equal	3
Number of men with the right and left ear equal	1

subjects, but one man was found whose left ear was better than his right in both series. Of the remaining 5 subjects whose left proved better than the right in second series, the right and left of two had been equal, the right of 3 had been slightly better than the left in the first series.

None of the 40 subjects knew of any defect in their hearing. They were asked if they knew of any difference between the

right and the left ear. Seven (2 woman and 5 men) thought the right was better; 5 (3 women and 2 men) thought the left better. None of these gave, or pretended to give, any good reason for their opinion; and in every case their opinion, when they thought better of the left, was not in accord with the results of the tests. The one man whose left ear proved to be better, knew of no difference.

The results indicating more acute hearing in the men cannot be due to a greater recklessness in answering. If this were true, we would expect the men to make more errors than the women when the fork was silent. On the whole they made fewer such errors. Many subjects made no errors of this kind. In the first series, 9 men made a total number of 46 errors, 20 with the left ear and 26 with the right; 9 women made 42 errors, 9 with the left and 33 with the right. In the second series the men were more cautious; 6 men made a total of 21 errors, 9 with the right, 12 with the left; 13 women made 68 errors, 29 with the right, and 39 with the left.

Many in this second series did not use the signal 'doubtful' at all. The number of such answers is given in Table IV. In this second series the left ear of both men and women was not only less acute, but more doubt was expressed and more errors made when it was used; the variation from the average is also slightly greater.

TABLE IV.

SERIES II.

Right ear, fork sounding.

45 'doubtful' answers were made by 14 women.
95 " " " " " 16 men.

Right ear, fork silent.

6 'doubtful' answers were made by 4 women.
2 " " " " " 2 men.

Left ear, fork sounding.

53 'doubtful' answers were made by 13 women.
116 " " " " " 16 men.

Left ear, fork silent.

No 'doubtful' answers made.

Fechner[1] found the left ear to be better than the right. His

[1] In *Poggendorf's Annalen der Physik und chemie*, vierte Reihe, Band III. S. 500.

method was to place a watch directly before the subject, who after closing with the forefinger first one ear then the other, stated in which ear the watch seemed louder. Such a method records only the subject's opinion. I found that results obtained by a test similar to Fechner's were not in accord with the results obtained by the tests with the tuning-fork. The subjects were the same 18 men and 18 women used in the second series of tests with the tuning-fork. A watch was held in front of the subject, who, after turning the head slowly from side to side, stated in which ear the watch seemed louder. Eight (4 men and 4 women) said the watch seemed louder in the left ear. The former tests seemed to show that the left ear of but one man was better; the right ear of the remaining 7 had been shown to be better. Five (1 man and 4 women) said the watch seemed louder in the right; the remainder of the subjects could tell of no difference. Again, I brought the watch from a point beyond to a point within the range of hearing, and asked the subjects with which ear they first heard the sound. The 8 subjects mentioned in the first test with the watch, answered that they heard it first with the left ear; and the same 5, who before seemed to hear it louder with the right, now said they heard it first with the right; the remainder of the subjects could tell no difference. There thus seems to be a slight tendency to think better of the left ear, even when that ear is the poorer. It is of course possible that if a tuning-fork had been used instead of a watch a different result would have been obtained.[2]

[2] The MS. of this article was received March 11, 1905.—Ed.

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EX1

EXTENSITY AND PITCH

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EXTENSITY AND PITCH.

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Psychologists in general agree that 'sensation' has the four characters of quality, intensity, duration (or protensity), and feeling-tone. To these some would add volume (or extensity), and others would add vividness. There seems to be no reason why the list should not be somewhat further extended by adding local significance, which may with advantage be distinguished from quality; and meaning, or symbolic value, which is at least as important as any of the other characters. Even with these additions there is question whether the list is complete, but it is sufficiently so for present purposes.

These characters of simple sense objects are not parallel in their functions, but differ in their relation to the existence of these sense objects, and in their modes of variation. The first three which we have named seem to be essential; we can not conceive of the existence of a 'sensation' which has no duration, or which has no intensity, or which has no quality; *i. e.*, which is no particular kind of 'sensation.' Whether the same holds true for extensity is perhaps less certain, but there is strong evidence that way, as we shall see later. Vividness, feeling tone, local significance, and meaning, seem to be less essential. There conceivably may be sense objects present to consciousness which yet are of zero vividness; that is, they are either in the realm of so-called subconsciousness, or else they are neglected in their immediate nature, and their meaning alone taken into account. Local significance, again, may possibly be lacking; olfactory objects, in particular, never seem to have in themselves any local signs, although of course they are localized by the help of tactual and muscular experiences; something of the same kind seems to be true of auditory sensations also. It may be that local signs exist only in touch and sight;

or on the other hand it may be that there is a system of local signs present in hearing which is represented by the system of pitches; but at least we can conceive of 'sensations' which should be entirely devoid of this character. Suppose, for instance, two nerve endings which should when stimulated produce sensual processes corresponding to the same sensuous quality; there might be in these processes nothing which would enable us to distinguish one from the other, and yet both would be real.

Sense objects may be neutral in feeling-tone, which is equivalent to their having no feeling tone. Some psychologists insist on calling neutrality a definite feeling-tone, thereby making feeling-tone an essential character; but this seems an uncalled for complication, since feeling-tone signifies the character by which states of consciousness differ as regards pleasantness (or unpleasantness, whichever you chose) and when this factor is reduced to zero there is no better excuse for saying that it still exists than there is for saying that zero intensity and zero duration are still existent intensity and duration.

Meaning, likewise, is not necessarily present. A sense object may conceivably be taken for just what it is, without reference to anything else. Of course, the more experience we have, the more nearly impossible this becomes; but even so we may hold that the absence of meaning does not imply the nonexistence of 'sensation.'

These various characters of simple sense objects show also diverse types of behavior in the analysis of psychical compounds. The mere quality of a compound involves nothing which is not in the qualities of its components. So the intensity and duration are direct functions of the intensities and durations of its elements. Or, if the more accurate form of statement is preferred, we may say that the qualities, intensities, and durations, of the elements into which the complex state is analyzed will include all qualities, intensities, and durations found in the complex, and no other; there is neither surplusage nor deficit resulting from the analysis. The complex as a whole has no quality which cannot be reduced to the qualities of the elements; no intensity which cannot be reduced to a summation of elementary intensities; and no duration which is different from the duration of a definite number of its elements.

Extensity falls in with duration. But in both these characters we must be prepared for an apparent surplusage resulting from analysis, which apparent surplusage is due to the phenomena of superposition; *i. e.*, simultaneity in time or collocation in space. When we take these factors into account the apparent post-analytical redundancy disappears.

With regard to the other characters of elementary sense objects, this simple analytic relation does not hold. There are factors in the feeling-tone of a complex that are not assignable to any of the sensuous elements into which the complex may be resolved, but which seem to belong to the complex as such, or perhaps to intellectual elements. The meaning of a complex is far different from the total meanings of its elements. Vividness is scarce amenable to analysis at all, for to a certain extent the vividness of the complex as a whole is inversely proportional to the vividness of its elements. Local significance may become entirely lost in a complex, especially when the complex involves the coöperation of two or more senses, and it appears in these cases only by the analysis of the complex into its elements.

With regard to their analytic behaviour therefore, as well as their essentiality, the feeling-tone, meaning, vividness, and local significance characters differ radically from quality, intensity, duration, and extensity.

There is however one way in which quality differs fundamentally from intensity, duration, and extensity, and that is in its method of variation. The variations in these last three are continuous from zero to the highest possible value, without any points of special value, *i. e.*, determining points, in the continuum; while the variations of quality pass through definite special or determining points with transition regions between. This difference is formulated in another way in the statement that variation in quality is variation in kind, while variations in intensity, duration, and extensity are not in themselves variations in kind.

As regards the physiological concomitants of at least five of these characters of simple sense objects, we can speak with confidence. Quality is essentially correlated with the kind

of end organ stimulated (or perhaps with the kind of process which is aroused in the end organ and brain cell). Intensity is correlated with the intensity of the process aroused. Duration is identical with the duration of the process, presumably in the central cell. Local significance is the correlate of the particular end organs stimulated; we may suppose each end organ to have its particular local sign. Extensity has in this respect its full title proven, since it is obviously the correspondent of the number of contiguous end organs stimulated.

The physiological conditions of the other characters — feeling-tone, meaning and vividness — are much more complex, and hence, though we assume that they are perfectly definite, are not readily assignable.

This scheme, of four *essential* characters, and four which may be called *accidental*, is satisfactory until we come to the consideration of sound sensations, at which point it seems at first unsatisfactory. Have we any character with which *pitch* can be identified? Duration, intensity, vividness, feeling-tone and meaning are of course excluded from the possibility, since they all apply to sounds over and above pitch; so quality, extensity and local significance are left to be considered. Quality, however, is soon put out of the running, since we have shown that in the other senses it varies through various determining points which are few in number for each sense; whereas pitch varies in a continuum without internal orientation, exactly as do intensity, duration, extensity and vividness. Local significance is the character which we should urge in this connection, if we held to the Helmholtz theory of audition, but since the present disposition is to look upon that theory as mechanically untenable, we should be obliged to look for further reasons for our choice, and such reasons, apart from the necessity of finding some character with which the identification may possibly be made, are not forthcoming. On the contrary, it seems impossible that pitch can rest upon local signs, since local signs do not in general vary between two extremes, but rather include a manifold of differences which do not admit of easy schematization. The same fact is expressed when we say that intensity, duration, extensity, and likewise pitch, admit of quantitative comparison, while local signs do not.

Extensity, however, furnishes conditions which correspond in every particular to the properties of pitch. It varies continuously between extremes, admits of quantitative estimation more or less exactly, and moreover is directly connected with pitch in introspective analysis. The so-called 'high' notes are *small.* The 'low' notes are *large* or *voluminous.* Differences in pitch, in other words, are directly comparable to differences in planar or linear extent, and the physiological condition of difference in pitch accordingly is probably difference in number of nerve-endings stimulated.[1]

This theory of pitch is much more in accordance with the known facts of tone perception than is the Helmholtz theory. Let us consider first the discrimination of overtones. According to the local sign theory there ought to be very little difficulty in recognizing the octave when sounded with the fundamental, for the two components are dependent on the stimulation of two nerve endings or groups of nerve endings which must be relatively a considerable distance apart in the series and hence as easily discriminable as two points of light on the retina. On the extensity theory, however, the nerve endings which the higher note stimulates are all stimulated by the lower note; that is, the higher note is contained in the lower note both psychologically and physiologically, just as if a short streak of light were superposed on a long one; so that the discrimination where the notes harmonize (*i. e.*, where there are no beats), and where the lower (or larger) note is not much less intense than the higher (or smaller), should be rather difficult, which is actually the case.

Moreover, if differences in pitch depended on differences in individual nerve endings stimulated, there would be no reason for expecting the lower note in a complex to dominate, *i. e.*, to give its pitch to the complex. But if the difference in pitch is really a difference in volume, we might well expect the larger to determine the size of the total complex, as really is the case when the lower note is not too weak.

[1] Subsequent to the construction of this theory on purely psychological grounds, I found that Ter Kuile had constructed a physiological theory with which it practically agrees. See *Pflüger's Archiv*, 1900, Vol. 79, pp. 146–157 and 484–509.

In the third place, as regards a changing pitch, we have on either theory something analogous to a perception of motion; but on the local sign theory the analogous motion would be that of a point generating a line in the field of vision or of touch; while on the extensity theory the proper analogy would be a line increasing or decreasing in length; *i. e.*, there is a certain part of the object which remains unchanged by the variation. This is a point on which introspection may differ in different cases, and on which it is hardly trustworthy on account of the sophistication of our auditory sense; in my own observation, however, the shrinking or expanding in linear extension in the field of vision or touch is a perfect analogue of the shifting pitch, and the moving point is not at all applicable.

Finally, the peculiarities of the complex sound we call 'noise, fit in perfectly with the proposed theory. The analytic characteristics of a 'noise' are: (1) excessive complexity, which is the essential feature, and may be the only one except for the beats to which it gives rise; (2) progressive variation in the intensity and pitch of the components, which is an accidental feature, but which adds greatly to the 'noisiness'; and (3) indefiniteness in pitch of the complex determined by the first two characteristics.

The reason for this indefinitness of pitch becomes clear as soon as we consider the analogy to light sensations. Suppose we superpose many streaks of light of various lengths, making one end of each streak coincident with the corresponding ends of all the others. The result will be a streak of light relatively very intense at one end and fading off at the other, so that the length of the total illuminated area is indefinite. This is just what happens in the case of a noise; the superposition of the variety of tones makes the exact limits of the complex difficult of determination, although there is a *general* pitch distinguishable.

The designation of pitch as the form which extensity takes in auditory sensation seems to be the most satisfactory clearing up of the field of sensation characters. Not only does it dispose of the vexed question of pitch, but it also helps to confirm the right of extensity to be considered an essential character of sensation, instead of an accidental character.

[1] The MS. of this article was received February 2, '05.—ED.

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FROM THE UNIVERSITY OF CALIFORNIA PSYCHOLOGICAL LABORATORY.

XI. EXPERIMENTS ON THE REPRODUCTION OF DISTANCE AS INFLUENCED BY SUGGESTIONS OF ABILITY AND INABILITY.

BY GRACE MILDRED JONES, M.L.

Up to the present time the several experiments to determine the effect of suggestion have been made with some important differences in the method of investigation. In the earlier experiments the observer was given no instructions to resist any influence by suggestion; as, for example, in Small's [1] test, where a visual illusion was employed and the children given no warning of such; so in Binet's [2] experiment where the subjects were misinformed as to the true length of the lines in their relation to one another. The experiments of Pearce,[3] and of Smith and Sowton [4] were made under practically these same conditions.

With Brand [5] a radically new method was adopted. The subjects were aware of the purpose of the experiment and while the content of the suggestion was to be given place in the mind the observers were warned against any voluntary response to it. Furthermore, the reactions depended not alone upon visual perception but primarily upon the power to reproduce. Bell's [6] experiment in these essential conditions was identical with Brand's. It differed chiefly in this: that a visual type of suggestion was used in addition to the vocal.

In the present investigation, the method in general remained

[1] Small, 'The Suggestibility of Children,' *Pedagog. Sem.*, 1896, IV., pp. 176–220.

[2] Binet, La suggestibilité, *Annee Psych.* V., pp. 82–152.

[3] H. J. Pearce, 'Normal Motor Suggestibility,' PSYCH. REV., 1902, IX., pp. 329–356.

[4] W. J. Smith and S. C. M. Sowton, 'Observations on Spatial Contrast and Confluence in Visual Perception,' *Brit. J. Psych.*, 1907, II., pp. 196–219.

[5] J. E. Brand, 'The Effect of Verbal Suggestion on the Estimation of Linear Magnitudes,' PSYCH. REV., 1905, XII., pp. 41–49.

[6] J. C. Bell, 'The Effect of Suggestion upon the Reproduction of Triangles and Point Distances,' *American Journal of Psychology*, 1908, XIX., pp. 504–518.

the same as with Brand. The nature of the suggestions was changed and instead of being given in the form of a command they conveyed ideas of ability and inability. The chief difference lay in the fact that three types of suggestion were used, the vocal, the visual, and the 'auto,'[1] and that a special investigation was made of the relative effect of these various types.

Method of Experiment.

The apparatus employed in the present experiment was almost identical with that used by Brand. At a distance of 80 centimeters from the subject two white pegs were set up showing through a narrow slit in a black screen; below this slit was another, somewhat wider, in which were exposed the visual suggestions. It was so arranged that the slits might be covered and uncovered conveniently to meet the needs of the experiment. At a distance of 40 centimeters from the observer was another black screen low enough so that the subject was able to see the horizontal slits in the farther screen where the pegs fixing the standard distance were exposed. On the nearer screen was a ledge where the subject was to adjust corresponding pegs in making his 'reproduction.' Both screens were built upon a table at a convenient level; the background was black and the room but dimly lighted. The pegs themselves were made clearly visible by screened lights.

The observer responded to six varieties of suggestion and to one signal where no suggestion was offered. Three types of suggestion were used—the visual, made by means of the printed mottoes "You are now able" and "You are now unable"; the vocal, made by the experimenter to the observer in the same words; and the 'auto,' made by the subject to himself in the words, "I am now able"; "I am now unable." The suggestions were given in irregular order; after each an interval of a second and a half was allowed before the exposure of the pegs, to give the subject time to concentrate his attention on the idea. The pegs were then exposed for a second and a half, the slide was replaced so as to conceal them, and the observer immediately

[1] 'Auto' is the name given to the type in which the observer responded to his own suggestion of ability or inability.

placed his pegs upon the ledge and adjusted them in accordance with his estimate of the distance between the exposed pegs. This distance, or space interval, was kept constant throughout the experiment; but of this the subjects were unaware, as their assertions prove. Each of the subjects took it for granted, when the absolute position of the pegs was changed, which occurred after every sixth judgment, that the space interval was changed also.

The experiment covered a period of five months, from October, 1908, to March, 1909; during this time the subjects were experimented upon at regular intervals and were required to give approximately the same number of judgments at each sitting. Three observers, experienced in psychological methods, were engaged. Five hundred twenty-five estimates were made by each of the three subjects; that is, seventy-five estimates for each variety of suggestion and seventy-five with no suggestion. The subject was instructed to allow each suggestion a place in his mind, to hold the idea it offered, but not to allow it consciously or intentionally to affect his estimate. Toward the close of the experiment each observer reported that he felt certain he was not being influenced in the least degree by any of the types of suggestion. According to their statements no feelings of ability or inability were aroused within them, nor was the experimenter able to observe any outward effect, such as hesitation or the like.

Results.

In that portion of Table I. which presents the averages for the combined seventy-five estimates the results are seen to be uniform for all the three subjects in these respects, that in all twenty-four 'groups'[1] where the affirmative of any type is compared with the negative of the same type, both the constant

[1] 'Group' is used here and throughout the account to mean a pair of averages, namely the affirmative and negative of any single type of suggestion or of the combined types — and this, either as regards average reproduction (or the constant error) or variability. For example, in Table I., in the first series of twenty-five judgments, 30.88 and 30.34 constitute a 'group' for subject *X*; 1.52 and 1.58 another group for the same subject. Thus for each series of twenty-five judgments there are eight groups for each subject, making twenty-four for the three subjects for that series.

error[1] and the variability[2] are appreciably less with the affirmative suggestions than with the corresponding negative suggestions. And again, in the averages obtained from judgments made with no suggestion there is always less constant error and usually less variability than in any of the averages of estimates with suggestion.

Table I.

The Average Reproduction of the Standard Distance (30 cm.) together with the Variability.

(The latter given in each case immediately below the average reproduction.)

Type of Suggestion.	Averages for First 25 Judgments. Subject			Averages for Second 25 Judgments. Subject			Averages for Third 25 Judgments. Subject			Averages for Combined 75 Judgments. Subject		
	X.	Y.	Z.	X.	Y.	Z.	X.	Y.	Z.	X.	Y.	Z.
Visual — affirmative	30.88	30.4	33.64	28.66	30.50	30.38	27.12	30.70	30.86	28.886	30.533	31.626
	1.52	1.64	3.68	1.74	.86	1.04	2.88	.94	1.42	2.046	1.146	2.046
Visual — negative	30.34	30.08	33.56	28.30	31.04	30.48	26.82	30.84	31.08	28.486	30.653	31.706
	1.58	2.04	3.72	1.9	1.48	1.36	3.18	1.08	1.56	2.22	1.533	2.213
Vocal — affirmative	30.94	30.06	33.18	29.16	30.64	30.06	27.75	30.68	31.06	29.28	30.46	31.433
	1.34	1.34	3.3	1.36	1.12	1.10	2.32	1.08	1.54	1.673	1.18	1.98
Vocal — negative	31.10	30.58	33.40	28.382	30.90	30.50	23.74	31.02	31.46	27.78	30.833	31.786
	1.9	1.66	3.64	1.7	1.1	1.54	2.26	1.38	1.62	1.953	1.38	2.266
Auto.— affirmative	30.72	30.06	32.96	28.46	30.48	30.16	27.74	30.88	31.76	28.973	30.473	31.626
	1.76	1.02	3.28	1.66	.84	.88	2.3	1.12	1.76	1.906	.993	1.973
Auto. — negative	30.88	30.28	32.88	28.54	30.50	30.70	27.40	30.74	31.48	28.94	30.506	31.686
	1.6	1.6	3.12	1.62	1.54	1.50	2.6	1.14	1.96	1.94	1.426	2.193
No suggestion	31.24	29.76	32.66	29.32	30.52	30.24	28.16	30.72	31.28	29.573	30.333	31.393
	1.72	1.72	2.74	1.24	.92	1.64	2.04	1.0	1.52	1.666	1.213	1.966
[3]All types of suggestion combined	30.81	30.243	33.27	28.583	30.676	30.38	26.761	30.81	31.283	28.724	30.576	31.643
	1.616	1.55	3.456	1.663	1.156	1.236	2.59	1.123	1.643	1.956	1.276	2.112
Affirmative suggestions combined	30.846	30.173	33.26	28.76	30.533	30.20	27.54	30.753	31.226	29.048	30.488	31.561
	1.54	1.333	3.42	1.56	.94	1.0	2.5	1.046	1.57	1.875	1.106	2.0
Negative suggestions combined	30.773	30.313	33.28	28.406	30.813	30.56	25.98	30.866	31.34	28.388	30.664	31.726
	1.693	1.766	3.49	1.74	1.373	1.46	2.68	1.2	1.71	2.037	1.446	2.224
Visual suggestions combined	30.61	30.24	33.60	28.48	30.77	30.43	26.96	30.77	30.97	28.686	30.593	31.66
	1.55	1.84	3.70	1.82	1.17	1.2	3.03	1.01	1.49	2.133	1.34	2.13
Vocal suggestions combined	31.02	30.32	33.29	28.77	30.77	30.28	25.75	30.85	31.26	28.513	30.646	31.61
	1.62	1.5	3.47	1.53	1.11	1.32	2.29	1.23	1.58	1.813	1.28	2.12
Auto. suggestions combined	30.80	30.17	32.92	28.50	30.49	30.43	27.57	30.81	31.62	28.956	30.49	31.65
	1.68	1.31	3.20	1.64	1.18	1.19	2.45	1.13	1.86	1.923	1.21	2.083

[1] The 'constant error' is the difference between the standard distance (30 cm.) and the 'average reproduction'—the latter being the average of the actual estimates made. Thus the 'average reproduction' for the three estimates 33, 28 and 31 would be 30.67; the 'constant error' 0.67.

[2] 'Variability' (=the 'crude variable error') was obtained by adding the amount of variation from the standard (30 cm.), regardless of its sign, and dividing by the number of cases considered; thus the 'variability' for the three judgments 33, 28 and 31 would be 2.

[3] "All types of suggestion combined" excludes throughout estimates with no suggestion.

Table II.

As Regards the Average Reproductions.

1. *Average Reproductions Compared with the Actual Distance Exposed.*

Subject.	First 25 Judgments.	Second 25 Judgments.	Third 25 Judgments.	Entire 75 Judgments.
X	Reprod. Dist. >	Reprod. Dist. <	Reprod. Dist. <	Reprod. Dist. <
Y	" " > (except 'no suggestion')	" " >	" " >	" " >
Z	Reprod. Dist. >	" " >	" " >	" " >

2. *Average Reproductions with no Suggestion Compared with Average Reproductions with Suggestion of any Kind.*

X	no sug. >	no sug. <	no sug. <	no sug. <
Y	" " < (except 3 affirm. cases and visual —)	" " < (except visual + and auto. + and —)	" " < (except visual + and vocal +)	" " <
Z	no sug. <	no sug. < (except vocal + auto. +)	no sug. < (except visual + vocal +)	" " <

3. *Average Reproductions with Affirmative Suggestion Compared with Average Reproduction with Negative Suggestion.*

X	vis. + > vis. — voc. + < voc. — auto. + < auto. —	vis. + < vis. — voc. + < voc. — auto. + > auto. —	vis. + < vis. — voc. + < voc. — auto. + < auto. —	vis. + < vis. — voc. + < voc. — auto. + < auto. —
Y	vis. + > vis. — voc. + < voc. — auto. + < auto. —	vis. + < vis. — voc. + < voc. — auto. + < auto. —	vis. + < vis. — voc. + < voc. — auto. + < auto. —	vis. + < vis. — voc. + < voc. — auto. + < auto. —
Z	vis. + > vis. — voc. + < voc. — auto. + > auto. —	vis. + < vis. — voc. + < voc. — auto. + < auto. —	vis. + < vis. — voc. + < voc. — auto. + > auto. —	vis. + < vis. — voc. + < voc. — auto. + < auto. —

As Regards Variability.

4. *Variability when there was no Suggestion Compared with that when there was Suggestion of any kind.*

Subject.	First 25 Judgments.	Second 25 Judgments.	Third 25 Judgments.	Entire 75 Judgments.
X	no sug. > (except vocal — and auto. +)	no sug. <	no sug. <	no sug. <
Y	no sug. >	no sug. < (except visual + and auto. +)	no sug. < (except visual +)	no sug. < (except visual +, vocal +, auto +)
Z	" " <	no sug. >	no sug. < (except visual +)	no sug. <

TABLE II. *Continued.*

5. *Variability when there was Affirmative Suggestion Compared with that when there was Negative Suggestion.*

X	vis. + < vis. —	vis. + < vis. —	vis. + < vis. —	vis. + < vis. —
	voc. + < voc. —	voc. + < voc. —	voc. + > voc. —	voc. + < voc. —
	auto. + > auto. —	auto. + > auto. —	auto. + < auto. —	auto. + < auto. —
Y	vis. + < vis. —	vis. + < vis. —	vis. + < vis. —	vis. + < vis. —
	voc. + < voc. —	voc. + < voc. —	voc. + < voc. —	voc. + < voc. —
	auto. + < auto. —	auto. + < auto. —	auto. + < auto. —	auto. + < auto. —
Z	vis. + < vis. —	vis. + < vis. —	vis. + < vis. —	vis. + < vis. —
	voc. + < voc. —	voc. + < voc. —	voc. + < voc. —	voc. + < voc. —
	auto. + < auto. —	auto. + < auto. —	auto. + < auto. —	auto. + < auto. —

In the portion of Table I. which presents the averages for the three successive series of twenty-five judgments, as we might expect, this general contrast in the effect of affirmative and negative suggestions appears somewhat less regularly; yet the general result of combining the entire 75 judgments under any one type of suggestion is seen to be fairly well distributed over these smaller series.

Thus in the averages for the first series of twenty-five estimates seven out of the twenty-four groups showed a reversed effect for the affirmative and negative suggestions, that is, the negative had less average error or variability than the affirmative; five of these irregularities were as regards the error, the other two as regards the variability. In the second series of twenty-five estimates three of the twenty-four groups were irregular, one in respect to the error, the other two in respect to the variability. In the third series of twenty-five estimates, three irregularities out of the twenty-four groups again occurred, two as regards the error and one as regards the variability.

Accordingly, out of the seventy-two groups, when the judgments were considered in series of twenty-five, there were thirteen irregular groups. Of these thirteen irregularities seven were with the 'auto' type of suggestion, three were with the visual, two with the vocal, and one appeared in the comparison of 'affirmative suggestions combined' and 'negative suggestions combined,' in the first twenty-five estimates.

As regards the smaller constant error in the estimates made with no suggestion than with those where there was suggestion,

the results apparent in the combined seventy-five judgments appear also in each of the series of twenty-five judgments, save for subject X in the first series. As regards the smaller variability with no suggestion, exceptions occur in both the first and second series of twenty-five judgments, but none in the third series.

In view of the fact that the judgments made under suggestion whether affirmative or negative show so frequently an increase in variability and error beyond that in the judgments made without suggestion, we may infer that suggestion does in itself, and apart from the actual 'contents' of the suggestion, effect some change in the reproduction of distance.

But it is also significant that the suggestion acts to a considerable extent in a direction corresponding to the actual 'contents' of the suggestion given; *i. e.*, the error and variability under suggestions of *ability* were almost always less than when suggestions of *inability* were made.

Previous experimenters found that one subject differed from another radically in the degree and nature of the change which suggestion produced. This is true only within somewhat narrow limits, in the present experiments. The three subjects showed the same significant tendencies in responding to suggestion; that is, with each, the suggestions of ability produced generally less error and less variability than did the suggestions of inability; and estimates with no suggestion were in each case still nearer the standard. The only difference lay in the fact that with two of the subjects there was a constant tendency to lengthen, with the other subject to shorten, the reproductions of the standard interval of 30 cm. And this constant error, whatever its direction, was increased by suggestions, especially by those of 'inability.' It would be unjustifiable to say that one observer showed more susceptibility to suggestion than another merely upon the ground of a different direction or a different absolute amount in his departure from the standard. Rather the subjects should be compared upon a basis of the *change* in the amount or direction of the errors or variability according as suggestions were present or absent, or according as one form of suggestion or another was employed. Upon this basis (see Table III.) there appears some difference between the three subjects.

Table III.

Order of the Effectiveness of Suggestion.

Based upon seventy-five judgments for each type of suggestion.

I. As Measured by the Change in the Average Reproduction.

(The types of suggestion decrease in effectiveness from left to right.)

Subject.							
X	Voc. —	Vis. —	Vis. +	Auto. —	Auto. +	Voc. +	No suggestion
Y	Voc. —	Vis. —	Vis. +	Auto. —	Auto. +	Voc. +	No suggestion
Z	Voc. —	Vis. —	Auto. —	Auto. +	Vis. +	Voc. +	No suggestion

II. As Measured by the Change in Variability.

(The types of suggestion decrease in effectiveness from left to right.)

Subject.							
X	Vis. —	Vis. +	Voc. —	Auto. —	Auto. +	Voc. +	No suggestion
Y	Vis. —	Auto. —	Voc. —	No sug.	Voc. +	Vis. +	Auto. +
Z	Voc. —	Vis. —	Auto. —	Vis. +	Voc. +	Auto. +	No suggestion

Bell in his experiment notes that the effect of suggestion decreased with repetition. This seems to be only partly, if at all, true here. If the fact that suggestions of ability produce less error, and suggestions of inability greater error in the reproduction of distance attests anything as to the power of suggestion this experiment shows that the susceptibility more regularly occurs with repetition, since as the experiment proceeds there occur fewer groups where this effect is reversed. Table I. shows seven irregularities in this respect in the first series of twenty-five estimates, and three each in the second and the third series of twenty-five estimates. Apart from irregularity, the absolute amount of the difference between 'affirmative suggestions combined' and 'negative suggestions combined' shows in the case of Subject *X* a uniform *increase* in the successive series of twenty-five judgments; and the same is true of the difference between 'no suggestion' and 'all types of suggestion combined.' With the other subjects there is neither a uniform increase nor decrease.

The purpose of the experiment was only in part to investigate the effect of suggestion in general; to determine the relative influence of the different *types* of suggestion was equally the purpose.

In Tables III. and IV. is set forth the relative strength or effectiveness of these different types of suggestion. It there appears that for each of the subjects the negative suggestions

TABLE IV.

Type of Suggestions.	Ratios of Reproductions with Suggestion to Reproductions Without. Based upon the Constant Error in Reproduction and also upon the Variability; Latter in Parentheses. Subject			Ratios of Reproductions with Suggestion to the Actual Standard (30 cm.). Based upon the Error in Reproduction. Subject		
	X.	*Y.*	*Z.*	*X.*	*Y.*	*Z.*
All types of suggestion combined. (450 reproductions)	.971 (1.174)	1.008 (1.051)	1.007 (1.074)	.957	1.019	1.054
Affirmative suggestions combined. (225 reproductions)	.982 (1.125)	1.005 (.911)	1.005 (1.017)	.968	1.016	1.052
Negative suggestions combined. (225 reproductions)	.959 (1.222)	1.010 (1.192)	1.010 (1.131)	.946	1.022	1.057
Visual suggestions combined. (150 reproductions)	.970 (1.280)	1.008 (1.104)	1.008 (1.083)	.956	1.019	1.055
Vocal suggestions combined. (150 reproductions)	.964 (1.088)	1.010 (1.055)	1.006 (1.078)	.950	1.021	1.053
Auto. suggestions combined. (150 reproductions)	.979 (1.154)	1.005 (.997)	1.008 (1.059)	.965	1.016	1.055

(suggestions of inability) were about twice as effective as were the positive suggestions (suggestions of ability). Moreover for each subject the vocal negative suggestions were the most effective of all as measured by the constant change in the average reproduction. As measured by the variability, the visual negative suggestions were stronger. With all the subjects the vocal positive suggestions, on the other hand, had the least influence upon the average reproduction; while upon the variability the 'auto' positive type was among those having the weakest influence.

As suggestions of ability and inability appeared to bring about an increase in the constant error and in the variability the question was raised as to whether the results were due merely to the *distraction* which the suggestions produced, and not at all to their inherent 'content' or ideas. It hardly seemed that such a theory could be fully justified, for it would not account for the fact that different types of suggestion had regularly produced different effects — effects consistent with the 'content' of the suggestion. However, seventy-five additional tests were made on each of the three subjects, in which the pegs were exposed and the reproductions made while the observer counted aloud the strokes of a metronome, swinging at five different rates, in order that the counting might more likely act as a dis-

traction and not become a merely mechanical operation. The other conditions were identical with those in the tests made for suggestibility.

Table V.

Results for Estimates made under Distraction.

Subject.	Average Reproductions of the Standard Distance together with the Variability.			
	Averages for First 25 Judgments.	Averages for Second 25 Judgments.	Averages for Third 25 Judgments.	Averages for Combined 75 Judgments.
X	28.48	28.32	28.12	28.31
	1.68	1.64	1.88	1.73
Y	29.86	29.38	29.15	29.48
	.94	.86	.93	.89
Z	29.18	29.14	31.65	29.92
	1.02	1.54	1.88	1.46

The variability is thus seen to be, on the average, less than when suggestions were offered, and for the most of the subjects less even than in the cases where no suggestion was made. As for the average reproductions, the two subjects whose estimate of the distance had been constantly greater than the standard in the previous experiment now had a tendency to make it less than the standard. The other subject whose tendency had been to underestimate the standard interval now had a tendency to make it still shorter.

The fact that the results with distraction differed in these respects from the results with different types of suggestion indicates that at least with two of the three subjects the peculiar effect of these suggestions is not due solely nor even predominately to any distraction which may have inhered in them.

12

THE EFFECT OF VARIOUS TYPES OF SUGGESTION UPON MUSCULAR ACTIVITY

BY EDW. K. STRONG, JR., M.S.

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XII. The Effect of Various Types of Suggestion upon Muscular Activity.

BY EDW. K. STRONG, Jr., M.S.

The purpose of the experiment here reported was to discover how far and in what manner maximum muscular activity may be affected by suggestions of various types. In all these the subject was instructed not to oppose any resistance to the suggestion nor on the other hand consciously to endeavor to carry out the suggestions, but his attention and conscious effort throughout was to be expended in exerting each time his maximum grip. Yet he was requested to maintain the suggestion in consciousness until after his muscular effort.

Historical Review.

There is scarcely any literature which bears directly upon the question under discussion in this paper. A number of articles[1] which have appeared lately indicate that some attention is being given the subject, but neither their methods nor their results have any direct relation to those of the present investigation. As far as I am aware Brand is the pioneer in this field of investigation. His experiment[2] was performed in this laboratory, and his purpose, as he states it, was "to find out how far and in what direction the visual estimation of a linear magnitude could be affected by suggestion of certain possible errors in such estimation, the subject knowing that the

[1] Triplet, 'Dynamogenic Factors in Pace-making and Competition,' *Amer. Jour. of Psych.*, IX., 507–533. Small, 'The Suggestibility of Children,' *Ped. Sem.*, IV., 182. Binet, 'La suggestibilité,' *L'année psych.*, V., 99. Pearce, 'Normal Motor Suggestibility,' Psych. Rev., IX., 348. Smith and Sowton, 'Observations on Spatial Contrast and Confluence in Visual Perception,' *Brit. Jour. Psych.*, II., 196–219. A very full discussion of all but the first accompanies the article of Bell ('The Effect of Suggestion upon the Reproduction of Triangles and of Point Distances') in the *Amer. Jour. of Psych.*, XIX., 504.

[2] 'The Effect of Verbal Suggestion upon the Estimation of Linear Magnitudes,' Psych. Rev., XII., 1905, 41–49.

suggestions were purely arbitrary, *i. e.*, that they had no reference to any foreseen tendency to err in any direction."[1]

The eight different suggestions used by Brand in his experiment were printed upon white cardboard in letters 1.2 cm. high. The conductor presented them to the subject by displaying the cardboard for a moment. After the suggestion had been presented the conductor displayed two small objects upon his frame and then called upon the subject to respond by setting up his two similar objects upon his own frame at a distance from each other approximating as nearly as possible that of the original objects.

I have reduced the results as given in his tables to percentages of the respective standards.

The following table gives the total results of Subject 'C,' eliminating the first group of experiments, since their variation is more than ten per cent. from the standard, due probably to lack of practice.

TABLE I.

Suggestion.	No. of Judgments.	Sum of Standards.	Result in per cent.
Zwp fjvic bgzx asye	49	1,258	100.5
Life is real where	49	1,258	99.5
Don't make too long	49	1,258	101.0
Don't make too short	49	1,258	101.0
Make short	104	2,632	99.5
Make long	104	2,632	102.0

And the following table gives the total results for Subject 'Y,' the first group of data having been likewise eliminated.

TABLE II.

Suggestion.	No. of Judgments.	Sum of Standards.	Result in per cent.
Zwp fjvic bgzx asye	49	1,086	91.0
Life is real where	49	1,086	90.0
Don't make too long	84	1,810	92.0
Don't make too short	84	1,810	92.5
Make short	100	2,400	94.0
Make long	100	2,400	94.0

[1] It is just at this point that suggestions as experimented upon in Bell's (see page 5) and Brand's work and in my own differ from the others. As Bell puts it: "The most potent factor without doubt in cases of suggestion is the arousal of an attitude of general expectancy." This factor has been utilized in the experiments of others, but is so far as possible eliminated in these three, for here the subject was particularly instructed that he was not to respond consciously to the suggestion but to endeavor consciously to exert his maximum grip.

From these tables I do not feel that any clear-cut deductions can be made, with the possible exception that the two brief suggestions 'Make short' and 'Make long' tend more than the other suggestions to make the reproduced distance greater in magnitude, and the two suggestions 'Don't make too long' and 'Don't make too short' tend to a less degree to have the same effect.

In Table III. Brand has brought together from group D (or the last group of experiments with both subjects) the totals for all the suggestions containing the word 'long' in one column and all those containing the word 'short' in another column, for comparison. The percentages as given below were inserted by myself.

TABLE III.

Subject.	No. of Judgments.	Sum of Standards.	'Long.'	Per cent.	'Short.'	Per cent.
'C'	58	1,628	1,702.3	104.5	1,644.4	101.0
'Y'	36	580	556.3	95.5	542.6	93.5

If compiled from all four of the groups instead of group *D* alone we have the following.

TABLE IV.

Subject.	No. of Judgments.	Sum of Standards.	'Long.'	Per cent.	'Short.'	Per cent.
'C'	207	4,932	5,095.8	103.0	5,052.2	102.5
'Y'	189	4,294	4,008.2	93.8	4,013.2	93.9

Here we have no superiority of the 'long' mottoes over the 'short' mottoes, except with the one subject.

Let us now turn to the second part of his experiment. Here but three suggestions were used: 'long,' 'short' and 'XXXX.' The latter was used as a check or neutral. Quoting from him: "Four subjects were employed and the results were not very uniform, two of the subjects showing no decided tendency towards anything resembling a constant effect, while the other two subjects showed a clear general constancy of considerable difference throughout."

In conclusion it seems to me that one cannot point with any emphasis to any direct effect from the above varied suggestions upon estimation of distance except (1) that 'long' suggestions may consistently affect one person more than corresponding

'short' ones; (2) that the reverse effect may be produced upon a second person; and (3) that there may be no appreciable effect of one or the other upon a third person.

In Bell's experiment[1] two types of suggestion were used — the auditory and the visual. For the former such as 'make high,' 'make low,' 'make high enough,' etc., or simply 'high' and 'low' were used, being spoken by the conductor just before the presentation of the object to be reacted upon. For the visual suggestion a diamond-shaped figure 20 cm. long and 4 cm. wide was shown to the subject. When displayed in a vertical position it was thought that it might serve as a 'high' suggestion, being much taller than any other figure shown. Likewise when shown in a horizontal position it was called a 'low' suggestion. (As far as I am aware the subject was not informed what this visual suggestion was intended to suggest. It may not then have had an equal effect upon the different subjects.)

The forms chosen for reproduction were: (1) triangles of different shapes and heights and (2) vertical point distances as shown by (*a*) a dot above the center of a base line and (*b*) a dot above another dot.

Under the first group (triangles of different shapes and heights) ten triangles were used, of the same base (10 mm.), but of different heights and shapes, and varying from 49 to 100 mm. in altitude. The work was so carried on that nine reproductions of each triangle were made with each kind of suggestion (*i. e.*, auditory and visual). Three of the nine were reproduced with 'high' suggestions, three with 'low' suggestions, and three without any suggestion. The altitude of the reproduced triangle was compared wiih the standard, and the difference expressed in millimeters as a positive or negative error. But the weakness of his experiment lies in the small number of observations per subject per triangle per suggestion, *i. e.*, *three* in number. Averages drawn from so few observations are hardly of greatest weight.

Bell concludes "that in general the suggestions do affect the reproduction of the triangles; that the auditory suggestion

[1] 'The Effect of Suggestion upon the Reproduction of Triangles and of Point Distances,' *Amer. Jour. of Psych.*, XIX., 1908, 504.

is more effective than the visual; and that in the auditory set the 'low' suggestion is more effective than the 'high.'" Upon looking at the subjects individually we find they have reacted differently. Subject 'B' alone showed striking susceptibility to the suggestion in all the cases. Subjects 'A,' 'C' and 'D' with the auditory suggestions and Subject 'E' with the visual suggestions, estimated the apex of the triangles below the standard when given either 'high' or 'low' suggestions. Throughout the experiment susceptibility to 'low' suggestions was more general and more uniform than to 'high' suggestions.

Under the second group (vertical point distances) the dot-above-line experiment was completed with auditory suggestions and the dot-above-dot experiment with visual suggestions. Bell in commenting upon his results states that there was little indication that the suggestions had any constant effect: with subjects 'C' and 'D' the 'high' and 'low' suggestions are both lower than the standard, the 'low' the lowest; while with subjects 'E' and 'F' the 'high' and 'low' suggestions are both higher than the standard.

Description of the Present Experiment.

The series of experiments carried on as described below can be divided into two parts, the first part consisting of those experiments which were performed during the months from September to December, 1908, and the second part consisting of those which were performed during the months from January to April, 1909.

The general plan of the experiment was to give the subject a suggestion, and then have him respond each time with his maximum grip. Collin's elliptical form of dynamometer was used and from it an expression in kilograms was obtained of his muscular activity.

The subject was seated in a chair which was provided with two flat arms about two and a half inches wide which extended as far forward as the front edge of the seat. The subject rested his arms from the elbow to the wrist upon these arms of the chair. When actually gripping the dynamometer the palm of his hand was uppermost. In the intervals he was allowed to

rest his hand as he chose. The conductor sat at a small table. On the edge of this table between the subject and the conductor a large upright black screen was placed in such a manner that the subject could not see anything upon the table; nor could he see the conductor.

Once every twenty seconds, until the series with one hand was completed, the subject gripped the dynamometer with his maximum force. The exact procedure was as follows. When the second-hand of the watch indicated the moment of presentation, the suggestion was given by the conductor. The dynamometer was then placed in the subject's hand. As soon as the latter had responded with his maximum grip the conductor took the instrument from his hand, noted the reading, and then awaited the next moment of presentation.

In all there were seven suggestions offered. They might be classified as follows:

I. *Auditory Suggestions.*

1. Positive. "Now you can make it stronger than usual."
2. Negative. "Now you can't make it as strong as usual."

These suggestions were presented vocally by the conductor.

II. *Visual Suggestions.*

1. Positive. A plus (+) sign 0.4 inches in size displayed upon a piece of cardboard two inches square.
2. Negative. A minus (—) sign 0.4 inches in size displayed upon a piece of cardboard two inches square.

In giving these suggestions the conductor placed the cardboard upon the corner of the table in front of the subject.

At the commencement of the experiment each subject was told that the plus (+) sign was meant to suggest to him that he could make his grip stronger than usual and the minus (—) sign was meant to suggest that he could not make it as strong as usual. These signs were consequently visual suggestions depending on previous vocal instruction.

III. *Auto-suggestions.* Here the conductor announced in the same tone and manner as in presenting the Auditory Suggestions "Now you can make a suggestion of your own." The subject understood by this that he was at liberty to suggest to himself either the positive or negative suggestion and to designate

his choice to the conductor by audibly announcing it. In this case as soon as the subject had announced his suggestion the dynamometer was handed him and the experiments continued as usual.

1. Positive. (After suggestion by the conductor, as above.) "Now I can make it stronger than usual," spoken by the subject.

2. Negative. (After suggestion by the conductor, as above.) "Now I can't make it as strong as usual," spoken by the subject.

IV. *Neutral 'Suggestion.'* This consisted of the announcement by the conductor of, 'Now, neutral,' and was intended to act merely as a check and guide to what would be the exertion if no suggestion of any sort were given.

In the earlier experiments a series consisted of fifty-six experiments or readings, twenty-eight with each hand. These twenty-eight in turn consisted of four each of the seven different suggestions. These twenty-eight were presented to the subject in a haphazard arrangement previously determined upon. All subjects were given the same haphazard arrangements and in the same order, so that direct comparisons between them could be made. The right hand in every case was experimented upon first. After four days' work with each subject, ten neutrals instead of four were introduced, thus making a day's work or series consist of thirty-four experiments instead of twenty-eight. Besides these twenty-eight or thirty-four experiments there were always two extra neutrals at the start which were for the purpose of practice and were always discarded.

A day's work with subject 'B' consisted of two series during one hour, — that is to say, of fifty-six experiments with each hand taken in this manner: twenty-eight with the right hand, then twenty-eight with the left hand, then twenty-eight with the right, and finally twenty-eight more with the left. A day's work with subjects 'J' and 'W' consisted of only one series during one hour. During the second term but one series per day was taken with any one of the three subjects. It should also be stated here that all three subjects were familiar with experimental work.

The procedure during the *second term* was exactly the same as in the first term except for the following. Instead of seven suggestions nine were employed. The two auto-suggestions were omitted and the following four added. Two auditory suggestions consisting of 'Now, plus' and 'Now, minus' to correspond to the two visual suggestions employed during the first term. Also two visual suggestions consisting of the mottoes: "Now you can make it stronger than usual" and "Now you can't make it as strong as usual" printed on cards 5.5 × 1.25 inches. These visual suggestions were to correspond to the auditory suggestions employed during the first term. Moreover the visual suggestions (+) and (−) were presented on cards similar to those just described, instead of on cards two inches square. Instead of 28 or 34 experiments on each hand per day there were 42 such experiments. These 42 consisted of four each of the eight suggestions and ten of the neutrals, arranged as before in a haphazard manner. During the second term the right hand was not experimented upon first each day, but only in alternation with the left hand.

The nine suggestions of the second term and the two auto-suggestions of the first term will be referred to in the tables by the following set of symbols.

I. *Auditory Suggestions* — in quotation marks.

'Can' means "Now you can make it stronger than usual."

'Can't' means "Now you can't make it as strong as usual."

'+' means "Now plus."

'−' means "Now minus."

II. *Visual Suggestions* — in brackets.

(Can) means the motto, "Now you can make it stronger than usual" printed on a card.

(Can't) means the motto, "Now you can't make it as strong as usual" printed on a card.

(+) means the symbol '+' printed on a card.

(−) means the symbol '−' printed on a card.

III. *Auto-suggestions.*

+ Auto means "Now I can make it stronger than usual," spoken by the subject.

− Auto means "Now I can't make it as strong as usual," spoken by the subject.

IV. *Neutral 'Suggestion.'*

'N' means "Now, neutral."

The Results.

In the following two tables there are presented the averages of 48 experiments for each type of suggestion with either hand of the three subjects, the numbers indicating in kilograms the maximal grip.

Table V.

Results for the First Term.

The numbers for the first six suggestions under Subjects 'B' and 'J' are an average of 48 results, and under Subject 'W' are an average of 24 results. The neutrals under Subjects 'B' and 'J' are an average of 96 results while under Subject 'W' they are an average of 36 results.

Suggestions.	Subject 'B.'		Subject 'J.'		Subject 'W.'	
	Right.	Left.	Right.	Left.	Right.	Left.
'Can'	38.8	29.6	17.2	15.0	33.1	23.7
'Can't'	38.4	28.9	17.1	15.4	33.3	23.4
(+)	38.9	29.9	17.7	15.0	33.8	24.4
(—)	38.6	29.3	17.1	14.8	33.5	24.4
+ Auto.	39.7	29.5	17.5	15.2	33.5	24.2
— Auto.	39.0	29.9	17.1	15.3	33.6	25.6
Average	38.9	29.5	17.2	15.1	33.5	24.3
Neutral	39.0	29.4	16.9	15.0	33.4	23.5
Average of all	38.9	29.5	17.1	15.1	33.5	23.9

Table VI.

Results for the Second Term.

The numbers for the first eight suggestions are an average of 48 trials while those for the neutrals are an average of 120 trials.

Suggestions.	Subject 'B.'		Subject 'J.'		Subject 'W.'	
	Right.	Left.	Right.	Left.	Right.	Left.
'Can'	40.3	31.8	16.9	15.9	32.8	25.1
'Can't'	39.7	31.4	16.9	16.3	32.6	25.4
(Can)	39.4	31.0	16.9	16.4	32.9	25.3
(Can't)	40.1	31.3	16.8	15.9	33.2	25.3
'+'	39.6	31.2	16.7	16.2	32.2	24.8
'—'	39.4	30.8	16.7	16.1	32.1	25.1
(+)	39.3	31.3	16.9	16.3	32.4	25.3
(—)	39.6	31.3	16.7	16.4	32.8	25.9
Average	39.7	31.3	16.8	16.2	32.6	25.3
Neutral	39.4	30.8	16.7	16.0	32.2	24.9
Average of all	39.5	31.1	16.8	16.1	32.5	25.2

From these two tables we see very clearly that suggestion as a whole heightens the maxima. In every case, except for the right hand of subject 'B' during the first term, the average of the neutrals for any of the three subjects with either hand is less than the corresponding average of all the other suggestions. The exception can probably be explained by an introspection of subject 'B' on March 1, when he stated that neutrals at *first* had a positive effect, that is to say, he felt that then was a time to make a 'record.'

In the following two tables we have a statement of the mean variation of the quantities entering into the preceding tables: Table VII stating the mean variation of the quantities in Table V, and Table VIII stating those of Table VI.

MEAN VARIATION TABLE VII. (FIRST TERM.)

Suggestions.	Subject 'B.'		Subject 'J.'		Subject 'W.'	
	Right.	Left.	Right.	Left.	Right.	Left.
'Can'	1.88	2.00	.86	.96	3.17	1.68
'Can't'	2.34	1.70	.74	.87	2.77	1.48
(+)	2.59	1.89	1.11	1.09	2.46	1.71
(—)	2.29	1.84	1.04	.83	2.62	1.70
+ Auto.	2.21	2.32	.93	.81	2.30	1.61
— Auto.	1.74	1.86	.93	.88	2.26	1.98
Average	2.17	1.93	.93	.91	2.59	1.69
Neutral	1.94	1.57	.85	.94	2.99	2.08
Average of all	2.14	1.88	.92	.91	2.65	1.75

MEAN VARIATION TABLE VIII. (SECOND TERM.)

Suggestions.	Subject 'B'		Subject 'J'		Subject 'W'	
	Right.	Left.	Right.	Left.	Right.	Left.
'Can'	2.58	1.54	.63	1.03	2.17	2.32
'Can't'	2.57	1.88	.73	1.01	2.62	2.31
(Can)	2.04	.80	.76	.91	2.06	2.19
(Can't)	1.80	.67	.67	1.02	2.38	2.39
'+'	1.82	.80	.79	1.08	2.32	2.24
'—'	2.17	.80	.77	1.13	2.61	1.97
(+)	2.46	.87	.81	1.20	2.11	2.04
(—)	1.97	.92	.89	1.13	2.11	2.06
Average	2.18	1.03	.76	1.06	2.29	2.19
Neutral	2.05	1.69	.85	1.12	2.37	2.50
Average of all	2.16	1.11	.77	1.07	2.31	2.22

In Table IX. we have a restatement of Table V., and in Table X. restatement of Table VI. to show the effect respectively of positive, negative and neutral suggestions upon the three subjects.

From these tables it is evident that the negative suggestions tend more than the positive suggestions with subject 'W' to heighten the maxima, and this is especially true with his left hand. But with the other two subjects the positive suggestions as a general rule are superior to the negative in this respect. However in all three cases the negative auto-suggestions with the left hand are clearly superior to the positive in heightening the maxima. Why this is the case is difficult to say. Possibly, the subject feels that after audibly declaring that he can't make it as strong as usual, he must exert greater effort in order to neutralize the suggestion's effect and actually obtain his maximum. With the right hand, however, this tendency does not appear, perhaps because of the hand's greater use and its consequent greater fineness in discrimination. Indeed it appears

Table IX.

Subject 'B.'

	With the Right Hand.			With the Left Hand.		
	Positive.	Negative.	Neutral.	Positive.	Negative.	Neutral.
Auditory	38.8	38.4		29.6	28.9	
Visual	38.9	38.6		29.9	29.3	
Auto.	39.7	39.0		29.5	29.9	
Neutral			39.0			29.4
Average	39.1	38.7	39.0	29.7	29.4	29.4

Subject 'J.'

	Positive.	Negative.	Neutral.	Positive.	Negative.	Neutral.
Auditory	17.2	17.1		15.0	15.4	
Visual	17.1	17.1		15.0	14.8	
Auto.	17.5	17.1		15.2	15.3	
Neutral			16.9			15.0
Average	17.3	17.1	16.9	15.1	15.2	15.0

Subject 'W.'

	Positive.	Negative.	Neutral.	Positive.	Negative.	Neutral.
Auditory	33.1	33.3		23.7	23.4	
Visual	33.8	33.5		24.4	24.4	
Auto.	33.5	33.6		24.2	25.6	
Neutral			33.4			23.5
Average	33.5	33.5	33.4	24.1	24.5	23.5

TABLE X.

SUBJECT 'B.'

	With the Right Hand.			With the Left Hand.		
	Positive.	Negative.	Neutral.	Positive.	Negative.	Neutral.
Auditory can–can't	40.3	39.7		31.8	31.4	
Visual can–can't	39.4	40.1		31.0	31.3	
Auditory + and —	39.6	39.4		31.2	30.8	
Visual + and —	39.3	39.6		31.3	31.3	
Neutral			39.4			30.8
Average	39.7	39.7	39.4	31.3	31.2	30.8

SUBJECT 'J.'

	Positive.	Negative.	Neutral.	Positive.	Negative.	Neutral.
Auditory can–can't	16.9	16.9		15.9	16.3	
Visual can–can't	16.9	16.8		16.4	15.9	
Auditory + and —	16.7	16.7		16.2	16.1	
Visual + and —	16.9	16.7		16.3	16.4	
Neutral			16.7			16.0
Average	16.9	16.8	16.7	16.2	16.1	16.0

SUBJECT 'W.'

	Positive.	Negative.	Neutral.	Positive.	Negative.	Neutral.
Auditory can–can't	32.8	32.6		25.1	25.4	
Visual can–can't	32.9	33.2		25.3	25.3	
Auditory + and —	32.2	32.1		24.8	25.1	
Visual + and —	32.4	32.8		25.3	25.9	
Neutral			32.2			24.9
Average	32.6	32.7	32.2	25.1	25.5	24.9

true that the so-called maximum effort in this experiment is not a real maximum exertion each time, but rather is an effort to attain a sort of definite standard. The left hand is thus at a disadvantage in maintaining its maximum, or definite standard, because of its lesser use and inferior discrimination.

In Tables XI. and XII. we have still a different restatement of Tables V. and VI. for the purpose of showing the effect of particular types of suggestion. In these tables the figure placed opposite each suggestion is the average of that type's positive and negative suggestions taken together; and the types are arranged according to the magnitudes of the averages.

We must conclude from these tables that: (1) the auto-suggestions tend most strongly of all the types of suggestion to heighten the maxima; (2) that during the first term the visual suggestions were superior in this respect to the auditory suggestions with subjects 'B' and 'W,' and were inferior with subject

TABLE XI.

WITH THE RIGHT HAND.

Subject 'B.'		Subject 'J.'		Subject 'W.'	
Auto.	39.35	Auto.	17.3	Visual	33.65
Neutral	39.0	Auditory	17.15	Auto.	33.55
Visual	38.75	Visual	17.1	Neutral	33.4
Auditory	38.6	Neutral	16.9	Auditory	33.2

WITH THE LEFT HAND.

Subject 'B.'		Subject 'J.'		Subject 'W.'	
Auto.	29.7	Auto.	15.25	Auto.	24.9
Visual	29.6	Auditory	15.2	Visual	24.4
Neutral	29.4	Neutral	15.0	Auditory	23.55
Auditory	29.25	Visual	14.9	Neutral	23.5

TABLE XII.

WITH THE RIGHT HAND.

Subject 'B.'		Subject 'J.'		Subject 'W.'	
Auditory Can-Can't	40.0	Auditory Can-Can't	16.9	Visual Can-Can't	33.1
Visual Can-Can't	39.8	Visual Can-Can't	16.9	Auditory Can-Can't	32.7
Auditory + and —	39.5	Auditory + and —	16.8	Visual + and —	32.6
Visual + and —	39.4	Visual + and —	16.8	Neutral	32.2
Neutral	39.4	Neutral	16.7	Auditory + and —	32.1

WITH THE LEFT HAND.

Subject 'B.'		Subject 'J.'		Subject 'W.'	
Auditory Can-Can't	31.6	Visual + and —	16.4	Visual + and —	25.6
Visual + and —	31.3	Visual Can-Can't	16.2	Visual Can-Can't	25.3
Visual Can-Can't	31.2	Auditory + and —	16.2	Auditory Can-Can't	25.2
Auditory + and —	31.0	Auditory Can-Can't	16.1	Auditory + and —	25.0
Neutral	30.8	Neutral	16.0	Neutral	24.9

'J,' while during the second term the visual suggestions were superior with subject 'W' and the left hand of subject 'J', and were inferior with the right hand of subjects 'B' and 'J'; and (3) that during the second term the motto suggestions (*e. g.*, "Now you can make it stronger than usual") were superior to the symbol suggestions (*e. g.*, plus sign) in heightening the maxima.

Before attempting any explanation of these results it should be borne in mind that Miss G. M. Jones, working at the same general problem outlined in this paper, and with two of the same subjects, but upon the reproduction of distance instead of upon maximal exertion, obtains the most accurate reproduction of distance with the neutral 'suggestion,' while the other suggestions,

instead of aiding in the reproduction of distance, apparently disturb the accuracy of such reproduction. (It is notable that all three subjects were affected nearly alike in her experiment while there were such different effects with different subjects in my own experiment, as well as in Brand's.)

Suggestion then affects the reproduction of distance by acting as a disturbing factor, but aids, as my results show, when applied to maximal muscular effort. From these two experiments it seems probable that when *accurate* work is to be done all suggestions prevent the best work; but when mere *amount* of muscular effort is called for, any arousal of the attention to the work acts as a stimulant and enables the person to do better than he would otherwise have done. This effect is also shown by an incident which occurred with subject 'W.' Near the close of the series with his left hand the experiments were interrupted by a messenger who notified him that he was awaited elsewhere. The eagerness to be through the experiment caused a rise of approximately five kilograms, or eleven pounds, in his grip for the remaining eight trials.

I have used the word 'attention' in this connection because I do not know a better, yet I am not entirely satisfied that it is a question of the attention, *i. e.*, that it is an intellectual arousal that causes the results described in this paper. That there is an arousal of the whole person is certain; but whether the heightened maxima, as in the case just cited, are due to the attention directly or to an indirect effect of the general stir, it is difficult to be certain. I do not feel that it is a question of the will primarily, for that factor is supposed to be eliminated from the experiment by the instruction at the commencement of work, when the subject was told to make his maximum effort each time.

Yet with these misgivings, it seems natural to explain the greater efficacy of the auto-suggestions by a greater concentration of the attention upon the work in hand. An auto-suggestion consisted of the statement by the conductor of the experiment, "Now you can make a suggestion of your own"; this was followed by the statement of the subject, "Now I can make it stronger than usual" or "Now I can't make it as strong as

usual." The whole process tended to call forth greater concentration of attention to the next grip of the dynamometer than would result from either the auditory or visual suggestion, where there was only an act on the part of the conductor.

On the other hand a certain broad arousal perhaps accounts for the marked superiority of the motto suggestions, "Now you can make it stronger than usual" and "Now you can't make it as strong as usual" over the symbol suggestions, '+' and '—.' The latter, especially when spoken ('auditory suggestion') occupied but a moment and did not arouse the subject as did the motto-suggestions, for the latter required a greater length of time to be understood.

Introspections of all three subjects were in general that the suggestions had little effect upon them. For example at the end of the second term subject 'B' once stated that the auditory suggestion 'can' seemed perhaps most powerful and the visual suggestion (+) was next, but that it did not seem as if any were strong enough to produce an effect. And about the same time subject 'J' stated her feeling as follows, "When I stop to think the suggestion seems to have little effect, but when not thinking I feel that I obey the suggestion." Throughout the experiment notes were taken of those cases in which the subject expressed himself as satisfied or dissatisfied with the result. A careful analysis of these fails to show any correlation between the actual result and the introspections.

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XIII. THE LOCALIZATION OF DIASCLEROTIC LIGHT.

BY G. M. STRATTON.

Some time ago Veraguth[1] announced that the sensation produced by stimulating the retina through the sclerotic coat instead of through the pupil is often localized upon the same side as that upon which the stimulus falls, and that this is particularly true when the stimulus falls upon the temporal side. With some persons, however, he found that under these circumstances there was, in addition to the sensation on the temporal side, a weaker sensation localized upon the nasal side. On the other hand, when the stimulus was applied to the nasal side the sensation was always localized in the normal way, that is, it was referred to the temporal side. When the light was applied to the sclerotic, not in a radial, or diametric, direction, but in a direction approximating that of the tangent at that point, no change was noticed in the place the light seemed to occupy in the visual field. From this fact Veraguth argues against the assumption that the light, when it falls upon the temporal side of the sclerotic, passes through and strikes the retina upon the nasal side and therefore is referred in quite normal fashion to the side opposite to that upon which it really affects the retina. He believes that we have here an indubitable departure from the common cross-localization of visual impressions, and would explain this departure by the biological principle of utility. As regards the need of a correct localization of light penetrating the sclerotic, there is, he feels, a great difference between the temporal side of the eye which is open and exposed, and the inner or nasal side which is screened by the nose. And he maintains that the correct orientation of light coming diasclerotically is important only when the light falls on the *temporal* side; on the nasal side the light would come not only through the sclerotic coat, but through the pupil, and therefore would

[1] 'Die Verlegung diaskleral in das menschliche Auge einfallender Lichtreize in den Raum,' *Zeitschrift für Psychologie*, Vol. 42, pp. 162 ff.

be localized correctly according to the well-known law. Pressure phosphenes, unlike many of these sensations produced by diasclerotic stimulation, are all projected, he finds, to the opposite side of the visual field; and this to him seems reasonable, because there is no advantage in their being projected otherwise. They therefore follow the general law for the projection of a stimulus which comes through the pupil.

Veraguth believes that the present phenomena are a stumbling block in the way of the nativistic theory of vision which supposes that the space value of the impression inheres in the retinal element. For, if this were the case, why should there be a difference between the localization of the sensation resulting from the diasclerotic stimulation and that from the diapupillary? But the empiristic theory, he argues, can well admit and explain these diverse facts, for it supposes that localization is due to a complex of factors, of which the retinal factor is but one. Now the diasclerotic and the diapupillary stimulation, he holds, may each arouse a different group of factors to determine its localization. Each may well be connected, for example, with a different group of muscular responses, and therefore, according to the empiristic view, have a different localization. With some persons it is not improbable that both the complex of factors concerned in diasclerotic localization and the complex concerned in diapupillary localization may come into play simultaneously, and thus there be brought to pass by diasclerotic stimulation a localization at once on both sides of the visual field. Thus he would explain the double projection which was sometimes noticed in his experiments.

To control Veraguth's data and explanation a number of experiments were tried by the present writer, at first by means of a very strong beam of artificial light in a dark-room and brought to a point on the sclerotic coat by means of a system of screens and lenses. Afterwards, a device essentially the same as that employed by Veraguth himself[1] was adopted. It consisted of a portable flash-light so covered at the end that the light was emitted only from a circular area less than one millimeter in diameter.

[1] Veraguth, "Zur Prüfung der Lichtreaction der Pupillen,' *Neurologisches Centralblatt* (16 April, 1905), XXIV. Jahrg., 338 ff.

This small opening lying well forward and beyond the body of the lamp could be brought close to the sclerotic coat of the eye, yet without actual contact. In this way the disturbing sensations, as well as the reflexes so difficult to suppress, were avoided. The observations were made by four persons; by two of these during frequent repetitions of the experiment over a considerable stretch of time. In general, these experiments while confirming much that Veraguth himself reports, yet bring some modification and supplement of his data, and lead, I venture to suggest, to a different conclusion.

There are regions upon the temporal side where the light is localized in the normal way; that is, upon the nasal side. There are regions where the light is localized entirely upon the temporal side. There is often found also a zone where there is some kind of localization upon both sides at once. The relation of these zones to one another is as follows: The region where the sensations are localized on the same side begins immediately posterior to the outer edge of the iris and extends backward a distance nearly equal to the distance from the center of the pupil to the outer edge of the iris. The region where the localization is upon the opposite side is still farther back. The region of double localization lies at the junction, or between the borders, of these two zones.

Upon the nasal side of the eye my own observations confirm, but not entirely, those of Veraguth. The localization is now far more frequently upon the side opposite to that upon which the stimulus falls, than in the case of temporal stimulation through the sclerotic coat. Yet occasionally there are found localities well forward where the sensation is localized upon the same side as the stimulus.

The localization of pressure phosphenes shows this peculiarity: that the phosphenes, so far as I can observe, are obtainable only upon those regions of the eye where light stimulus, passing through the sclerotic coat, arouses what I should call a more definite, a more figurate or punctiform sensation. In the anterior region where the application of light upon the sclerotic gives a vague sensation of light, local pressure upon the sclerotic coat produces no phosphenes whatever. The phosphenes,

however, when produced, are always localized upon the side opposite to that of the incidence of the stimulus, as Veraguth himself observed. By test experiments in which there was carefully noted the angle in the visual field where the extreme outlying phosphenes are localized, I find them ceasing to appear at about the outmost limit for the perception of light coming through the pupil. This would tend to confirm one in the belief that the retinal limit for response to actual light is the same as that for pressure stimulation.

Veraguth has laid considerable emphasis upon the fact that no change in the localization of the sensation takes place when, instead of having the stimulus come to the outer coat of the eye in a diametric direction, it is introduced in a direction approximately that of the tangent at that point. It does not seem, however, that this fact should be regarded as of great importance. It *would* be of importance were the outer coats of the eye perfectly transparent; but the sclerotic, being translucent, would in any event diffuse the light much as would tissue-paper or egg-shell; that is to say, the light would be transmitted in all directions from the point of incidence more or less indifferently, whether the light itself, in arriving at that point, came in one direction or another. In consequence, we should expect that if the sensation itself were in anywise due to the passage of light into the bulb of the eye, and over to the retina upon the opposite side, it would not be affected by a change from diametric to tangential incidence.

Another fact which must be taken into account in the explanation of the experimental data is that there is a retinal zone of considerable width anterior to the *ora serrata* which contains neither rods nor cones.[1] Yet this is a region where light, falling upon the sclerotic coat, nevertheless produces light sensations. And, furthermore, the present experiments lead me to believe that this is the region especially liable to the production of the diffused sensations which are localized upon the temporal side, both by temporal and by nasal stimulation. It would seem, to

[1] See Piersol, *Human Anatomy*, 1907, pp. 1456, 1467; Tolot, *Anatomischer Atlas*, 1907, p. 892; Werner Spalteholtz, *Hand Atlas of Human Anatomy*, tr. Barker, III., 772, 780; Huber, *Text-book of Histology*, tr. Cushing, 1900, p. 422. I am indebted to my colleague, Dr. Moody, for assistance upon this point.

express it mildly, anomalous to explain the localization of light here as though there were actual light-sensitive elements directly beneath the region of the sclerotic where the stimulus falls.

There is, one must confess, some difficulty at the present time in giving any explanation of all the facts which appear in this interesting experiment. Yet on the whole there would seem sufficient reason to believe that Veraguth's theory is forced and improbable.

In the first place, it is hardly in keeping with other well-known facts of retinal behavior to suppose that the stimulation of the very same retinal elements will lead to such different localization by reason of some change in the manner of *approach* to those elements. The stimulation of the rods and cones by pressure, for example, — a stimulation which also comes through the sclerotic, and consequently by a course entirely different from that of ordinary photic stimulation — occasions no upset of the normal localization of these elements; their sensation is referred to exactly the same place as though it were caused by light, and had come through the pupil.

The facts perhaps are better explained by assuming that the light which penetrates the sclerotic coat *posterior* to the *ora serrata* and which consequently excites the light-sensitive elements of the retina directly beneath or adjacent produces the definite or figurate sensation which is localized upon the opposite side. This is the region where the localization of phosphenes and that of sensations produced by light stimulus are identical. But where the light falls *anterior* to the *ora serrata* it of course cannot reach immediately and upon the same side of the eye light-sensitive elements but can stimulate them only after first being diffused through the interior of the bulb. The character of this diffusion will be such that the light will pass in straight lines from its point of incidence upon the sclerotic, but in lines traversing the interior of the bulb in *all* directions and not just in the direction which is a continuation of that along which it was travelling before its incidence. The behavior of the light here is such as one would get in a dark room if a beam of light fell upon a plate of ground-glass in an aperture of the

wall. There would be a general illumination of the room but naturally more intense upon the side opposite to the illuminated spot, yet diffused and unfigurate upon that side.

This would account for two facts: namely, that the sensation in the case of such light stimulation as falls very far forward on the sclerotic is exceedingly indefinite, and is localized upon the same side as that upon which the light itself has been introduced into the eye. Furthermore, it would account for the fact, already referred to, that a change in the direction along which the light falls upon the sclerotic does not alter the character of the projection, provided the point of incidence remain unchanged. The position of greatest illumination in our dark-room would not be altered perceptibly by altering the direction along which the light proceeded to the plate of ground-glass.

The behavior of the sensation produced by light falling on the nasal side one must confess is mildly puzzling. For the most part all stimuli here, no matter how far forward they fall, are referred to the opposite side; yet we should expect that if we go forward of the *ora serrata* on this side we should find that the sensations were referred exclusively to the nasal side. The facts here obtained may perhaps be accounted for by the fact that the *ora serrata* upon this side reaches farther forward[1] than upon the opposite side, in accord with the familiar observation that the field of view upon the temporal side is always wider than upon the nasal. Consequently the stimulus introduced diasclerotically upon the nasal side would almost inevitably find light-sensitive elements adjacent to, if not directly beneath, the point of incidence; and the sensation thereby produced would by its intensity quite drown any sensation arising from a diffusion inside the eye. It is possible also that this phenomenon of irregular localization is to some extent occasioned by a reflection of light from the surface of the crystalline lens perhaps near its periphery. This surface of the lens might easily reflect the light back to the same side as that upon which it entered the sclerotic, though to a point somewhat farther back. Thus it would reach the sensitive portions of the retina and now quite normally be localized upon the opposite side.

[1] See, *e. g.*, Werner Spalteholtz, *op. cit.*, III., p. 780.

All cases where there is a double localization from a single stimulation, that is to say, a localization at once upon both sides of the visual field, could well be accounted for by supposing that the light, after passing through the sclerotic, reaches not solely those light-sensitive elements which lie in the portions of the retina immediately beneath or adjacent, but is also diffused to the opposite side of the inner chamber and there falling diffusely upon the retina is referred to the side opposite to that upon which the more definite sensation is localized. We should thus have two spacially disjoined sensations occurring from a single impression because the stimulus actually fell upon disjoined portions of the retina.

My own feeling therefore is that nothing appears in these experiments with diasclerotic light which is at all discordant with the law governing the localization of diapupillary impressions. Visual localization is based upon the positions which objects normally occupy when stimulating the different points of the retina; and this localization, once established, takes no account whatever of the course by which the stimulus happens actually to arrive. No questions are asked as to whether it has entered by the door or has broken in as a thief and a robber; it is treated in all cases psychically alike.

FROM THE UNIVERSITY OF CALIFORNIA
PSYCHOLOGICAL LABORATORY

XIV. The Psychology of Change: on Some Phases of Minimal Time by Sight

BY JOHN M. BREWER, B.S.

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BY JOHN M. BREWER, B.S.

The present paper is concerned with the description of an experiment that attempts to bring nearer the answer to the following questions: (1) What is the smallest time-interval between two successive lights when the lights are seen to come not simultaneously but in succession? (2) What is the smallest extent of time that a motion can last, and still be perceived as motion? And the investigation of these two questions, with the hope of bringing out some connection or disconnection of the one group of facts with the other, was confined to a special set of conditions; namely, that in which the two points whose succession or simultaneity is to be observed, are not in absolute juxtaposition but are appreciably apart, and where the space-interval between them is varied as we pass from group to group of experiments. For it seemed an open question whether an advantage that might accrue, let us say, to either motion or succession, when one space-interval was involved, would also be maintained with a different interval of space.

The subject, in a somewhat darkened room, was placed 360 cm. distant from a large screen bearing a narrow vertical slit that could, for the sake of proper fixation, be dimly seen. And in this position he saw either two lights in succession, one directly above the other, or else a moving point of light. These were produced in the following way. Behind the screen and directly in line with the slit was a surface evenly lighted by an electric light; and between screen and lighted surface swung a pendulum, bearing a wide sheet of metal, set into which were devices for producing the appearances

described above. For giving an appearance of succession two small apertures were cut in separate slides that were in reality circular arcs so arranged that the apertures would pass the slit with a varying intervening time, yet with no varying of their distance from the point of suspension of the pendulum; and consequently for any given set of experiments they presented to the observer always the same angular, or vertical, separation. Yet in the different sets of experiments the amount of their vertical separation differed, being respectively 7, 10 and 19.5 mm., or, expressed in angular measure, 6.7′, 9.5′ and 18.6′ of arc.

Instead of the appearance of succession, it was possible to offer an appearance of motion by substituting for the apertures just described a circular card that could be moved at will about its own center. In this circular card there was a narrow slit; and by different settings of the card its slit could be placed at any angle to the slit in the stationary screen immediately before it; and consequently there was presented to the observer the appearance of a point of light moving either up or down. The extent of the motion up and down was carefully limited by opaque surfaces above and below, and made to correspond, in the three parts of the experiment, with the spatial angles above mentioned: 6.7′, 9.5′ and 18.6′. The velocity of the apparent movement, of course, varied step by step with the shifts in the angle of the pendulum slit. The swing of the pendulum was kept constant by the use of magnets, and its speed in the vicinity of the center was carefully calibrated by means of a tuning-fork.

In determining the thresholds for the different orders of stimulation, the method of serial groups[1] was used, 'no-interval' cases being intermixed about equally with the intervals whose thresholds were sought. The threshold was crossed after the manner of the method of minimal changes, and for both orders of sequence (viz., upper light first, and lower light first), and of motion (viz., downward and upward). Each value in Tables I.–III. is then an average of four

[1] See 'Studies from the Univ. of Calif. Lab., IV.,' Psychol. Review, IX., 1902, 444.

'determinations' and into each determination there enter forty to fifty experiments. And since it was often the case that an observer could tell that the lights came at different times, but could not tell which came first, or observed motion, though he could not tell its direction, the two kinds of threshold are here recorded separately—the threshold for the

Table I

Thresholds, Expressed in σ, for Visual Angle 6.7′

Observer.	With Discrete Sequence.		With Motion.	
	Judg. of Succession Merely.	Judg. of Order of Succession.	Judg. of Motion Merely.	Judg. of Direction of Motion.
B	22	29	29	31
L	22	22	21	21
M	54	60	48	48
S	20	22	30	32
W	33	43	22	30

Table II

Thresholds, Expressed in σ, for Visual Angle 9.5′

Observer.	With Discrete Sequence.		With Motion.	
	Judg. of Succession Merely.	Judg. of Order of Succession.	Judg. of Motion Merely.	Judg. of Direction of Motion.
B	22	30	22	22
L	32	32	19	19
M	58	61	33	33
S	35	35	28	32
W	33	33	21	21

Table III

Thresholds, Expressed in σ, for Visual Angle 18.6′

Observer.	With Discrete Sequence.		With Motion.	
	Judg. of Succession Merely.	Judg. of Order of Succession.	Judg. of Motion Merely.	Judg. of Direction of Motion.
B	37	39	30	30
L	28	30	20	20
M	53	60	76	76
S	45	45	24	24
W	43	52	20	20

bare perception of the succession or motion, and that for the definite order of the succession or for the direction of motion. It should be said that the subjective appearance of motion, which Exner noticed, did not appear to any important

extent in making these determinations. Only one subject had this illusion, and he for a very short time only. The results are stated in thousandths of a second.

There is in many cases, as these tables show, a lower threshold when motion is offered than when there is offered discrete sequence. In the thirty pairs of values that are comparable in this respect, twenty-one show a clear advantage for motion. Yet in none of these pairs is there any such advantage as Exner noted. Moreover, it is evident that the smallest visual angle is for the greater number of the observers the most favorable of the three for the perception of the discrete sequence, whether the judgment passed be of sequence merely or be of the order of the sequence. The separation of the lights would here seem to be sufficient to prevent at least any troublesome degree of overlapping of the irradiation circles upon the retina, and at the same time not so great as to render comparison difficult. At the angle 9.5′, such comparison seems already to have become harder and at 18.6′ harder still. For the perception of motion, however, the most favorable space extent for most of the observers was neither the shortest nor yet the longest of the three employed, but the middle distance. Motion through the angle 6.7′ seems to have been too short to be most readily seen as motion; while with the angle 18.6′ there evidently was again some difficulty, though of a different kind and greater—that again prevented the distance from being optimal.

Some idea of the comparative advantages of the different angular extents may be gained from the following table wherein the thresholds for the different orders of perception are stated in the form of averages of the time obtained for all five of the observers.

TABLE IV

AVERAGE THRESHOLDS, EXPRESSED IN σ, FOR THE THREE VISUAL ANGLES

Character of Change Presented.	Visual Angle 6.7′.		Visual Angle 9.5′.		Visual Angle 18.6′.	
	Judg. of Occurrence Merely.	Judg. of Order or Direction of Occurrence.	Judg. of Occurrence Merely.	Judg. of Order or Direction of Occurrence.	Judg. of Occurrence Merely.	Judg. of Order or Direction of Occurrece.
Discrete sequence	30	35	36	38	41	45
Actual motion	30	32	25	26	34	34

So far as discrete sequence is concerned, there are, however, exceptional observers, like M, who reveal no great difference as we pass from one visual angle to another; or, like B and W, for whom 6.7′ and 9.5′ are practically alike, while 18.6′ shows a distinct disadvantage. And, again, in the region of motion, the general advantage possessed by the medium extent 9.5′ practically disappears with the observer W for whom there is, if anything, a very slight drop in the thresholds as we pass out to the very largest angle, that of 18.6′; and with another observer, S, this same tendency is even more clear, and the longest distance, 18.6′, is now the most favorable of all. Yet the departure of these observers from the precise relations found with their fellows permits one still to say that there is in general in shorter space-extents a condition *favorable* to the perception of discrete sequence, and yet *unfavorable* to the perception of motion. And while the most favorable extent here used for perceiving discrete sequence gives, on the average, a minimal threshold of 30σ, the most favorable extent for perceiving motion gives a threshold of 25σ; the threshold for the perception of discrete sequence is thus about one fifth higher than that for motion. This, however, shows that at least under the peculiar conditions of the present experiment, there is a considerable departure from the relation which Exner believed to exist, namely, that the time-threshold for the perception of succession is about three times as high as that for the perception of motion.

FROM THE UNIVERSITY OF CALIFORNIA
PSYCHOLOGICAL LABORATORY

XV. THE PSYCHOLOGY OF CHANGE: HOW IS THE PERCEPTION OF MOVEMENT RELATED TO THAT OF SUCCESSION?

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XV. The Psychology of Change: How is the Perception of Movement Related to that of Succession?

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The experiments to be described deal with a narrow margin of that wide process by which we are conscious of change. For even with all the interest that has in past years been shown in this complex act, one can hardly say that there is as yet any general agreement as to its inner character or as to its outer connections. It has repeatedly been asserted that change is, at its best, caught by us directly and simply; that when we notice some sudden brightening of a light or damping of a sound or increase of pain or shift of place, the change-experience itself is of a unique quality, like the taste of bitter or the sight of blue, and that at such times the process has all the marks of a sensation. The detection of change, it is held, involves at such times no comparison, no overspanning consciousness that now the object is in one state or condition and now in another, but we become aware of the change in a single and simple throb of mind. And all this has been denied.

In the case of visual movement, to be more specific—for with visual movement alone the present experiments are concerned—orthodox belief has been that there is always present a double act of discrimination, at once of instants and of positions; against which is the belief that the retina is directly responsive to rapid movements of light, and that the movement is perceived without any such complex dealing at once with space and time. The idea of an absolutely direct and simple sensory appreciation of motion suggested by Czermak[1] and to some extent furthered by Vierordt,[2] found a strong

[1] 'Ideen zu einer Lehre von Zeitsinn,' *Sitzungsb. d. kaiserl. Akad. d. wiss. Math.-Naturwiss. Cl.*, Vol. 24 (Vienna, 1857), pp. 231–236.

[2] 'Der Zeitsinn nach Versuchen,' 1868; cf. especially pp. 114 f., where Vierordt says that in perceiving motion both the time-sense and the space-sense are required, but that the processes involving space and time cease to arise clearly in consciousness because they run so smoothly and habit-like, and so the whole takes on a quasi-sensory character. Accordingly, says he, we may as justly use the expression *Geschwindigkeitsempfindungen* as *Zeit- und Raumgrössenempfindungen*.

supporter in Exner, whose argument rests upon two kinds of experimental observation, in the region of space and of time respectively. In regard to space, rapid motion can be discerned, he held, within limits too narrow for space-discrimination; a small disk of light can be seen to move even when its excursion is considerably less than the distance between two such disks of light that, when stationary, already appear as one. *Consequently* (we may paraphrase his argument) *space-discrimination cannot be an essential part of the process of perceiving motion*, at least when rapid. Furthermore, and now passing over into the region of time, motion can be seen as motion even when its duration is far less than the least time that permits us to notice succession. If instead of making the excursion through a shorter and shorter distance, the extent of the excursion remain constant and the movement be made more and more *rapid*, Exner found that, even when the motion occupied a time so brief that two sparks, the one at the beginning, the other at the end of such duration, seemed absolutely simultaneous, the motion was still perceptibly motion. *Consequently* (so runs the thought) *time-discrimination cannot be an essential part of the process of perceiving motion.*[1]

As regards the first part of this argument—the part that deals with space-discrimination—I may perhaps refer to experimental evidence already offered by myself tending strongly (so it seems to me) to show that the facts upon which the argument is based were gathered far too narrowly.[2] If, instead of relying on the two-point method of determining the space-threshold, we employ a method involving the apparent dislocation or coincidence of two parallel lines placed end to end, the power to detect differences of position is approximately the same as that for the perception of movement. And consequently a full half of the ground for believing in the psychic simplicity and independence of movement caves away.

[1] Cf. the argument as set forth in James's *Principles of Psychology*, II., 172. The refs. to Exner are found below.

[2] 'Visible Motion and the Space-threshold,' PSYCHOLOGICAL REVIEW, IX., 433 ff.; and cf. Bourdon: *La perception visuelle de l'espace*, pp. 144 f., with the refs. there given.

I. Previous Experimental Work

Exner's experiments upon the time-factor in the perception of visual movement were in substance as follows.[1] Using close-neighboring electric-sparks, he found that with eyes at a certain distance from the sparks the most rapid succession that could be discerned was one of 44σ. But by more than doubling the distance between eye and sparks and thus proportionately diminishing the visual angle between the sparks and at the same time producing an illusion of movement from one to the other, he found this movement still noticeable when the time-interval was shortened by practice to 15σ. Substituting for the electric sparks flashes of light through two apertures in a screen, Exner could tell correctly the order of sequence when the flashes were 45σ apart; but when the portion of the screen between the two apertures was cut out and otherwise so arranged as to give a moving light between these two localities, motion could still be perceived when its duration was but 14σ. Motion, whether real or apparent, thus had power, it seemed, to reduce the limit below which a visual stretch of brightness appeared as a synchronous and uniform illumination. Exner's data for this conclusion were obtained from foveal vision; with eccentric vision he found it impracticable to exclude the appearance of motion; he was thus unable to compare in this region the times of just-visible motion and of succession apart from movement.

Charpentier, who next in any significant way approached the problem, employed a method much the same as the second of Exner's described above, yet with this important difference, —that instead of producing discrete succession by separated, although neighboring spots of light, he arranged a vertical slit, of which he could illuminate independently the upper and the lower half, and thus the sensations whose sequence or simultaneity was to be caught appeared in absolute juxtaposition. He found: (1) that if the time-interval between the halves be slight, they seem illuminated simultaneously;

[1] Exner, 'Experimentelle Untersuchung der einfachster psychischen Processe Pflüger's *Archiv*, VII., 601 ff.; VIII., 526 ff.; XI., 403 ff.; 'Ueber das Sehen von Bewegungen und die Theorie des zusammengesetzten Auges,' *Sitzungsb. d. kaiserl. Akad. d. Wissens. Math.-Naturw. Cl.*, LXXII., Pt. 111 (Vienna, 1875), pp. 156 ff.

(2) with increase of this interval the illuminations seem distinct in time, but it is impossible to say which comes first; (3) only with still greater increase of the time-interval can one say which is first; and (4) the absolute duration of the illumination of each half seems not to affect this detection of sequence. On the average (and apparently from but a few experiments) he found a noticeable difference when the beginnings of the lights differed by 27σ.[1] With regard to Charpentier's discussion of the error we make in the appreciation of succession—the illusion that successive lights are simultaneous—it was contended by Bloch that this error entirely disappears if the successive stimulations do not fall upon identical parts of the retina,[2]—a surprising criticism, apparently based neither on any right understanding of Charpentier nor upon any reliable experiment of Bloch's own. Charpentier, in reply,[3] after clearing up the matter of method, says briefly and with restraint, that while he cannot deny that the succession may continue to appear to the very end, he is quite unable to notice it himself. Neither Charpentier nor Bloch seem to have made more than a few trials nor to have concerned themselves with obtaining comparable data in the region of movement, and so of clearing up the problem more directly before us.

This problem was, however, held distinctly in mind by Stern,[4] who arrives at a conclusion opposed to that of Exner. But since his own experimental contribution is confined to the spatial rather than to the temporal side of the question,[5] it hardly need here be reproduced.

[1] Charpentier, 'Sur l'appréciation du temps par la rétine,' *Séance et Memoires de la Societé de Biologie*, Vol. IV., 8th Series (Paris, 1887), pp. 360–1; 'Nouvelle note sur l'appréciation du temps par la rétine,' *ibid.*, pp. 373–5.

[2] Bloch, 'Note sur les sensations visuelles,' *ibid.*, pp. 391-3.

[3] Charpentier, 'Note sur le synchronisme apparent,' etc., *ibid.*, pp. 447–8.

[4] 'Die Wahrnehmung von Bewegungen vermittelst des Auges,' *Zeitschrift für Psychologie*, etc., VII. (1894), 321 ff. Stern is in the neighborhood of the time-side of the problem only when he considers the most favorable *rhythm* of motion if the motion is to be noticed. Cf. my own discussion and opposing results, in 'Visible Motion and the Space Threshold,' Psychological Review, IX., 433 ff.

[5] This is true also of the work of Basler, 'Ueber das Sehen von Bewegungen,' Pflüger's *Archiv*, 1906, XV., 582, 1908; CXXIV., 313.

Weyer, experimenting with successive electric sparks, given (it would seem) from a single sparker and consequently appearing at one identical point, brought out some interesting facts concerning the detection of sequence; but his interest was not directly in our problem, and he made no attempt to obtain in the field of movement data with which his results upon succession might be compared. He found, however, that in starting with so brief an interval that the two sparks appeared as one, there were certain definite intermediate stages before this appearance of precise singleness of spark yielded to a clear appearance of succession. The apparently unitary illumination began to drag, or 'persist' when the interval was somewhat increased—to about 12σ, with daylight adaptation. With still greater increase—to an interval of 25σ–30σ—there came a slight flicker. Finally, when the interval was increased to 43σ–53σ, the two sparks appeared in clear succession.[1]

With Bourdon there was distinct interest in our particular problem, and he accordingly gathered data both of succession and of movement. He used small areas of white paper attached to a revolving drum, so arranged that they produced an appearance now of motion and now of mere sequence of neighboring lights. When the conditions were on the whole most favorable, he found motion recognizable by direct vision at a duration of 27σ. With discrete succession instead of motion, he found it impossible to pass judgment when the interval was only 16σ; his right judgments exceeded the wrong when the interval was 20σ; when the interval was increased to 48σ, he recognized with certainty the *order* in which the lights disappeared. His conclusion is that the perception of motion has no such preëminence over that of succession as Exner claimed; indeed, the thresholds of these two kinds of perception come to nearly the same measure.[2]

[1] Weyer, 'Zeitschwellen gleichartiger und disparater Sinneseindrücke,' Wundt's *Philosophische Studien*, XIV., 616; XV., 67.

[2] Bourdon, *La perception visuelle de l'espace*, pp. 187 ff. Marbe (*Theorie der kinematographischen Projectionen*, 1910, p. 62) has added stlll later experiments, but again upon but one of the two sides important for the present question. He finds that when a series of 13 miniature incandescent lights flash in succession at a rate wherein the interval between the flashes of adjacent lights is reduced to 12σ the impression of movement ceases.

Only Exner and Bourdon, then, maintained in the two regions that must of necessity here be compared, a single unchanging method applied under inner and outer conditions that also in the main remained unchanged. And their excellent work, I have ventured to think, still leaves room, even apart from a certain conflict of outcome, for some doubt and inquiry. With Bourdon, for instance, it is clear that exactly the same conditions were not maintained both in his experiments upon succession and in those upon motion. The lines that passed the slit were not alike in the two cases; and this might have affected the apparent intensity of the sensations; the slit through which the stimulus appeared was, in the case of the motion, horizontal and 1 mm. wide, in the case of succession it was vertical and 0.4 mm. wide; the distance between the observer and the lights was different in the two cases. All these discrepancies should be removed, and the two series to be compared should run on abreast to avoid any possible advantage from the difference of training and practice. In particular it has seemed to me desirable to regard as an open question what the effect of certain variations of method would be; and farther, just what space relation should subsist between the successive lights, if the conditions were to be most favorable for the detection of succession. Will succession best be observed when the successive spots of light are closer together or farther apart; when they are in juxtaposition or when some distance intervenes? We should discover the most favorable conditions for each class of observations before we could finally decide as to their relative superiority.

It would be idle to claim that the programme finally adopted covers the ground; all that should be said is that it aims to clear up an obscure point or two. A part of it has been carried out by an able student of mine, Mr. Brewer,—the part dealing with successions that are not only temporally but spatially separate. And his report accompanies and may be regarded as an intimate part of this discussion.[1] His experiments had shown a regular reduction of the time-

[1] Brewer, *The Psychology of Change: On some Phases of Minimal Time by Sight*, in the present number of the PSYCHOLOGICAL REVIEW.

threshold for succession, as the visual angle was reduced. For myself, I felt tempted to push this reduction to its very limit by making trial, in the region of time, of a slight modification of method that has proved of marked effect in studying movement upon its spatial side. For it will perhaps be remembered by some that a change in the manner of testing the space-threshold, whereby lines were placed end to end, instead of side by side, had reduced the space-threshold at least five-fold and perhaps ten-fold; had reduced it from a visual angle of 30″ with stars, or of 60″ if we keep to lines, to a visual angle of approximately 7″.[1] Here in placing the lines end to end, there is perfect proximity of the impressions to be compared, yet without the easy fusion which befalls lines side by side. An analogous procedure in the realm of time would be to have the succession of flashes come, not upon the same points of the retina where persistence of impression would conceal the time-interval, but upon adjacent points, yet neither with confusion by overlapping circles of irradiation nor with any insistent illusion of movement. This seemed attainable by means of a narrow slit, half of which should be lighted at one instant, and half at another,—the time-interval between these illuminations to be variable and definitely known. The plan, then, involved the method already employed briefly by Charpentier, and at the same time the gathering of exactly comparable data regarding movement.

II. Experiments with Pendulum Device

In the present experiments two different arrangements of apparatus were employed. The first group of experiments was with the pendulum described in Brewer's paper already mentioned;[2] but with attachments other than those he used. Imagine a plate of aluminum just above the pendulum's bob, lying in the plane of oscillation. In this plate and at about a distance of 1 m. from the point of suspension of the pendulum there was a vertical radial slit 5 cm. long, with a mean

[1] 'A New Determination of the Minimum Visibile,' Psychological Review, VII., 249; 'Visible Motion and the Space-Threshold,' Psychological Review, IX., 433 ff. Cf. Bourdon, *La perception visuelle de l'espace*, pp. 144 ff.

[2] See pp. 25 f.

width of 4.5 mm. later narrowed to 1 mm. The pendulum with this radial slit swung behind a stationary screen which also had a vertical slit with parallel sides 2 mm. apart and long enough to expose the full length of the slit behind. Back of both screen and pendulum was ground glass evenly illuminated by six 16-c.p. incandescent lamps. With certain adjustments the observer, at a distance of 2 m., perceived a momentary illumination of the vertical slit in the stationary screen; while with other adjustments the upper and lower half of the radial slit in the pendulum could be mutually so displaced as to present to the observer the two halves of the slit illuminated in quick succession. The exact point in the front slit where these two halves had their common border was, for the sake of preparatory fixation, indicated by white horizontal arrows, while the time-interval between the illumination of the two halves could be exactly controlled by a scale obtained chronographically.

The impression of movement, for the companion experiments, was obtained by another attachment essentially the same as that described in Brewer's paper.[1] It need hardly be said that here the same width of slits and intensity of illumination of background, was maintained as in the experiments on succession, and the extent of the motion was identical with the length of the two illuminated halves then used.[2] And in all cases the two sets of experiments were so arranged as to exclude any difference of practice or fatigue in the two groups. There were two observers in the present group of experiments, as well as in a second group later to be described. To Dr. Brown, who gave unstintingly of time and good will in these none-too-interesting tests, I acknowledge my great indebtedness.

Since the tendency here is to see every impression as motion and indeed as motion in some particular direction,

[1] See p. 258.

[2] Brewer's experiments, it will be seen, showed that the optimal excursion for perceiving *motion* is, unlike that of succession, by no means near zero. With some it seems to lie at least as far out as a visual angle of 18.6′. The motion in the present experiments was some five times this extent, and in all probability included the optimal excursion for my observers.

the 'method of serial groups'[1] was used instead of the more common gradation method. The procedure was '*unwissentlich*'; and but a single movement, or pair of lights in succession, was offered for judgment. At first an attempt was made to distinguish, in each judgment, not only between motion and succession but also between the two orders of succession (upper first; lower first) and the two directions of movement (down; up). But prepossession soon proved too strong, and so wide a range of intervals was needed to catch all phases of these judgments that I finally abandoned the attempt to obtain thresholds for the correct perception of the order of

TABLE I

OBSERVER B

Thresholds expressed in mm. (*1 mm.* = *1.5σ*)

Order of Presentation	With Two Successive Lights		With Moving Light	
	Above-below	Below-above	Downward	Upward
With wider slit.	12 11 18 15	17 17 15 15	35+ 35 45	25 30 25
Average.	14	16	38.3	26.7
With narrower slit.	15 20	20 22	40 35	25 20
Average.	17.5	21	37.5	22.5
Combined average.	15.2	17.7	38	25
Average for the two orders of presentation.	16.4		31.5	

succession and for the direction of motion, which are unquestionably distinct from the thresholds for the perception of mere succession and mere motion. I felt content to note the point at which actual succession on the one hand and motion on the other, no longer seemed sheer simultaneity of illumination. Yet the observers were, through a large part of the present group of experiments, more or less concerned to make out more exactly the nature of whatever departure from simultaneity they noticed, and so their judgments were not quite untrammelled and homogeneous. Yet they should perhaps be entered, since in several ways they are worth

[1] See PSYCHOLOGICAL REVIEW, IX., 444 ff.

comparing with the group to be spoken of some pages hence. Each 'determination' given in the tables accompanying (Tables I. and II.) represents never less than 50 nor more than 100 separate judgments. In all, there were between 1,500 and 1,600 judgments in this group, apart from many preliminary experiments and the half dozen judgments at the opening of each hour for 'warming-up.'

Table II

Observer S

Thresholds expressed in mm. (1 mm. = 1.5σ)

Order of Presentation	With Two Successive Lights		With Moving Light	
	Above-below	Below-above	Downward	Upward
With wider slit.	9 8 10 10	20 20 20 20	10 15 10	20 20 15
Average.	9.2	20	11.7	18.3
With narrower slit.	12 13	13 23	8 8	15 17
Average.	12.5	18	8	16
Combined average.	10.3	19.3	10.2	17.4
Average for the two orders of presentation.	14.8		13.8	

The tables show that for Observer B. there was a decided advantage on the side of succession, and for Observer S., no marked advantage either way, although a slight advantage appears on the side of motion. Whatever gain here accrues to 'succession' is not in any wise due to an illusory motion when mere succession was offered. On the contrary, with both observers the presentations, whether of succession or of motion, when just rising above the threshold were perceived as *succession* far more often than as motion. The usual course of the judgments was this; with the longer times there was a clear discrimination between succession and motion; and then, with briefer times, the presentation was called 'successive' or 'different,' meaning that it was not a single simultaneous illumination. Even with actual motion, therefore, the observer failed to notice the motion, but noticed

only non-simultaneity. And this lower threshold is entered in the tables even in the case of motion. Where, with B. for example, the thresholds for motion are entered as 30σ and 35σ, this indicates the point at which a departure from simultaneity was evident; motion was discerned as motion only at 40σ and 45σ respectively; and with S. the detection of motion required a value even higher in its ratio to that of mere succession.

Furthermore, there is a difference, in some cases quite marked, between the thresholds for the different orders of presentation. Thus for both B. and S. the threshold is lower when, in the case of succession, the upper half of the slit is illuminated before rather than after the lower. And with motion also the direction affects the threshold, although here the two observers do not show a like divergence, since B. best catches motion upward, while with S. motion downward has the advantage. But of this phenomenon more will be said in connection with the later experiments.

III. Experiments with Wheel Apparatus

The set of experiments just reported seemed to me inconclusive because of a growing doubt both of the apparatus and of the method. The puzzling disagreement of results obtained in the different orders of presentation, though the observer did not have to decide which order was given him (to which attention has just been drawn), made me perhaps unwarrantably inclined to believe that this inconsistency might be due to some want of rigidity in the instrument. And hesitation arose, too, from the very fact that the values obtained bore, at least in the case of one of the two observers, a relation so decidedly the reverse of that in the classic results.

Consequently an entirely new apparatus was constructed by a skilful instrument-maker—a pendulum-wheel suggested by a device of Sanford's,[1] but departing from it in several respects. From a sheet of aluminum ¼ in. thick, there was cut a narrow circular rim 20 in. in diameter, having thin poke-like connections with its center. At the center there was a steel axle with ball-bearings resting upon a rigid frame and base,

[1] Sanford, *American Journal of Psychology*, VI., 581 ff.

upon which there were levelling screws and a circular spirit level. To the rim of this skeleton wheel was carefully fitted a weight and catches, so that with the weight high upon one side, when one of the catches was released, the wheel revolved until the weight rose high upon the opposite side; and there at its extreme excursion the wheel was caught and the return of the weight prevented. In preparation for the next fall, the rotation of the wheel was completed by hand, and by means of the catch the weight was held high upon the side whence it first fell.

Upon the axle of the wheel was a screw holding two large black bristol disks, one of which was fixed by additional screws upon the rim, the other being movable and to be set instantly at any position desired. In these disks there were in principle, the same devices for producing succession and movement as with the pendulum before. Thus any order and interval of succession, any direction and speed of movement, could be had at will. Proper scaling in thousandths of a second was provided beforehand by tuning-fork records on the disk, the tuning-fork itself having of course been first carefully corrected.

The method was such as to avoid any extraneous advantage accruing, by practice or fatigue, to either of the forms of perception. Experiments upon succession and upon motion were from the first mingled in equal number at every sitting; nor did the observer himself know in any case whether the stimulus to be offered him involved motion or succession merely, nor was he told this after his judgment had been given. But a single movement, a single pair of successive lights, were given for any one judgment. And further it seemed advisable, in view of what had happened in the preceding set of experiments, to simplify the task of the observer and render his judgments more direct and trustworthy. Hitherto, as already has been indicated, the judgments showed some embarrassment before the several possible queries: (1) Was the impression throughout its stretch simultaneous, or was there some departure from perfect simul-

taneity? (2) If there was such a departure, had it the character of a successive illumination of the two halves of the slit; or of a motion? (3) If a succession, did the lower half of the slit flash before or after the upper? (4) If a motion, was this upward, or down? Now the observer is ill at ease before so many questions, and his nicety of judgment is apt to be affected by the attempt to keep his mind at once open to so many aspects of the impression, and in the effort to answer secondary questions the answers to primary are often obscured or lost.[1]

Accordingly in this whole group of experiments, even from the start, the observer was asked to confine his attention to the first of the four questions just listed. Additional judgments were occasionally volunteered; and although noted, they were not encouraged. The method again was, at bottom, that of serial groups, inasmuch as a number of fixed intervals or durations were selected and repeated; and interspersed with these were frequent cases where the interval or duration was reduced to zero. Yet instead of adhering to a particular interval or duration until its group was complete, and exchanged for a new group and so on until the threshold had been passed, there was no attempt to determine a threshold in the accepted sense, but only by a full display of right and wrong cases, with each interval or duration, to reveal the relative facility of perceiving movement and succession. The following partial series of actual stimuli presented, where the distribution is quite by chance, will perhaps make clear the procedure just described.

0σ motion (=simult. illum. of entire slit).
10σ succession, upper-lower.
20σ motion, upward.
30σ succession, lower-upper.
0σ succession (=simult. illum. of entire slit).
10σ motion, upward.
0σ succession.
25σ succession, upper-lower.

[1] See the argument offered by Brown ('The Judgment of Difference,' *University of California Publications in Psychology*, I., 28 ff.) for a simple alternative in the question—an argument that has had great weight with me.

Table III

(Judgments upon Single Presentations; Direct Vision)

Observer B

Discrete Succession

Interval[1]	Successes	Failures
30σ	12	0
25σ	12	0
20σ	13	1
15σ	10	4
10σ	10	4
5σ	6	8
Totals.	63	17

Motion

Duration	Successes	Failures
30σ	8	4
25σ	9	3
20σ	10	4
15σ	6	8
10σ	2	12
5σ	6	8
Totals.	41	39

Of 72 instantaneous illums., 50 were judged 'simult.,' 22 'non-simult.'

Table IV

(Judgments upon Single Presentations; Direct Vision)

Observer S

Discrete Succession

Interval	Successes	Failures
30σ	10	2
25σ	11	1
20σ	12	0
15σ	9	3
10σ	5	7
5σ	6	6
Totals.	53	19

Motion

Duration	Successes	Failures
30σ	12	0
25σ	9	3
20σ	10	2
15σ	11	1
10σ	7	5
5σ	6	6
Totals.	55	17

Of 72 instantaneous illums., 47 were judged 'simult.,' 26 'non-simult.'

[1] 'Interval' here and elsewhere means the time between beginning and beginning, or end and end, of the two successive flashes.

20σ motion upward.
15σ succession, lower-upper.
5σ motion, downward.
0σ succession.
30σ motion, upward.
0σ motion, etc.

The following tables exhibit the results of these experiments.

Method of Paired Presentations.—Before discussing these results, let me offer also those of another method, used as a check. Here the apparatus and procedure was in all respects as just described, except that there was given, in sequence, two presentations; namely, a zero case—where there was absolute simultaneity of illumination—and a case where there was actual succession or motion. The observer was told that there would be two presentations, one of which would be a simultaneous illumination of the entire slit, the other a departure from such simultaneity; and that his task was simply to tell whether the simultaneous illumination occurred first or second in the pair. The two presentations were then given, with as little time between as could be with the adjustment required (about 4 seconds), and no judgment was passed until after the appearance of the second of the pair. The

TABLE V

(JUDGMENT BY PAIRS; DIRECT VISION)

OBSERVER B

Discrete Succession

Interval	Successes	Failures
20σ	11	1
15σ	10	2
10σ	8	4
5σ	6	6
Totals.	35	13

Motion

Duration	Successes	Failures
20σ	7	5
15σ	8	4
10σ	8	4
5σ	4	8
Totals.	27	21

Table VI

(Judgment by Pairs; Direct Vision)

Observer S

Discrete Succession

Interval	Successes	Failures
20σ	14	0
15σ	10	4
10σ	11	3
5σ	11	3
Totals.	46	10

Motion

Duration	Successes	Failures
20σ	13	1
15σ	13	1
10σ	7	7
5σ	6	8
Totals.	39	17

observer was ignorant of the actual order, and this order itself was irregularly changed. The following tables exhibit the results obtained.

Examination of the results of these two different procedures discloses that with neither of the observers, nor by either method, is there an advantage for motion over succession. And consequently the new apparatus and simplified task for the observer brings the same general relation of values as were obtained in the first set of experiments with the pendulum. Observer B. now, as before, can more readily detect a departure from simultaneity in the illumination when departure actually is of succession rather than of motion. Observer S. again, for a part of the set—when there were single presentations—shows about equal facility with either kind of stimulus; but with double presentations shows greater ease in dealing with successsion.

Any one interested in method, and in the psychological indications arising from differences therein, will perhaps feel inclined to compare more closely the outcome by single presentation (Tables III. and IV.) and by double (Tables V. and VI.). It will be seen that Observer B. has, so far as discrete succession is concerned, a slightly greater success by the method of double presentation, and with motion he shows

a decidedly higher ratio of successes to failures when the presentation is in pairs. Observer S., when discrete sequence is offered, succeeds markedly better with pair-presentations than with single-presentations; when motion is offered, the ad-

Table VII

Observer B. Direct Vision

With Discrete Succession

Time Interval	Judgment by Single Presentation		Judgment by Double Presentation	
	Successes	Failures	Successes	Failures
200σ	13	1	11	1
150σ	10	4	10	2
100σ	10	4	8	4
50σ	6	8	6	6
Totals	39	17	35	13
Ratio of successes to failures	2.3 : 1		2.7 : 1	

With Motion

Duration	Judgment by Single Presentation		Judgment by Double Presentation	
	Successes	Failures	Successes	Failures
200σ	10	4	7	5
150σ	6	8	8	4
100σ	2	12	8	4
50σ	6	8	4	8
Totals	24	32	27	21
Ratio of successes to failures	0.75 : 1		1.3 : 1	

Table VIII

Observer S. Direct Vision

With Discrete Succession

Interval	Judgment by Single Presentation		Judgment by Double Presentation	
	Successes	Failures	Successes	Failures
200σ	12	0	14	0
150σ	9	3	10	4
100σ	5	7	11	3
50σ	6	6	11	3
Totals	32	16	46	10
Ratio of successes to failures	2 : 1		4.6 : 1	

With Motion

Duration	Judgment by Single Presentation		Judgment by Double Presentation	
	Successes	Failures	Successes	Failures
200σ	10	2	13	1
150σ	11	1	13	1
100σ	7	5	7	7
50σ	6	6	6	8
Totals	34	14	39	17
Ratio of successes to failures	2.4 : 1		2.3 : 1	

vantage for the method in which pairs of presentations occur disappears. The following tables (VII. and VIII.) in which the comparable values are isolated and set side by side and summed, will, I trust, make this more clear.

In general it would appear that a greater fineness of perception is possible by the method of paired presentation than by that of single presentation. And furthermore there is noticeable in the values obtained by the method of paired presentations a somewhat greater regularity; the number of successes or of failures keeps better step with the change in the time-interval, the correlation between the interval employed and the success of perception is closer when this method is employed than when its rival.

Experiments with Eccentric Vision.—Such was the evidence upon the main problem, so far as a foveal vision is concerned. Experiments were tried also with eccentric vision, the observer fixating with both eyes a point 20° to the left of the slit in which the illumination appeared. Tables IX.

Table IX

Experiments with Eccentric Vision

Observer B

Time	Discrete Succession		Motion	
	Successes	Failures	Successes	Failures
300σ	11	1	10	2
250σ	8	4	10	2
200σ	6	6	6	6
150σ	8	4	8	4
Totals.	33	15	34	14

TABLE X

EXPERIMENTS WITH ECCENTRIC VISION

Observer S

Time	Discrete Succession		Motion	
	Successes	Failures	Successes	Failures
30σ	4	0	4	0
25σ	3	1	4	0
20σ	11	1	11	1
15σ	9	3	9	3
10σ	9	3	6	6
5σ	9	3	4	8
Totals.	45	11	38	18

and X. show the results of these experiments. The presentations were always by pairs; the procedure was in all other respects as last described.

Under these conditions it is apparent that, with Observer B., there was almost equal success with either kind of stimulus, although the values indicate in both cases a higher threshold than with direct vision. With Observer S., there is somewhat greater facility in dealing with succession than with motion, although here the values indicate no rise of the threshold. Thus in this region as in others, succession is certainly at no disadvantage as compared with movement.

Absolute Time in the Results.—As for the absolute time-values obtained in these experiments, they run considerably lower than we have been accustomed to regard as obtaining here. It will be recalled[1] that Exner found 15σ to be the duration needed for perceiving motion; 45σ, for discrete succession. Bourdon recognized motion at 27σ; with discrete succession right judgments exceeded wrong at 20σ; the time order of succession was caught with certainty at 48σ. Weyer found a slight drag at 12σ, a 'flicker' at 25σ–30σ, clear succession at 43σ–53σ. Charpentier discerned succession at 27σ.

In the portion of the present experiments where the vision was direct and the conditions otherwise were most favorable, Observer S., even with so brief an interval as 5σ, distinguished succession from simultaneity 11 times in 14 trials (*i. e.*, 80 per cent. of the judgments were correct; *cf.* Table VI., p.) 277. B.,

[1] See pp. 263 ff. of the present paper.

when the interval was 10σ, correctly made this distinction in 10 of the 14 cases offered him (70 per cent.; *cf.* Table III., p. 275, and Table V., p. 276, where two thirds of B.'s answers were correct, with 10σ). And, again, with indirect vision, an interval of but 5σ still permitted Observer S. to give correct judgments regarding discrete succession in 9 cases out of 12 (75 per cent.; *cf.* Table X., above.

Such a lowering of the time-threshold to about one third of what usually has been considered its lowest value (to 5σ, as against 15σ), if motion be regarded as offering the finest measure of the time-threshold; or to about one ninth of its accepted value (to 5σ, as against 45σ) if succession be taken as criterion,—this lowering is to be ascribed to a combination of causes: perhaps (1) to the method of presentation, inasmuch as examples both of simultaneity and of non-simultaneity, it was known, would be offered, and the observer had simply to affirm which was which; and (2) to the instrumental arrangement,—whereby there was given an immediate juxtaposition of successive lights, yet without fusion—an arrangement analogous to the successful 'vernier-method' in experiments with space; and (3) to practice, since with each subject a far greater number of experiments was completed than has been usual in this field.

Some might feel tempted to say that the lowering of the values arose from the fact that the judgments here given were not due in the first instance to time-perception at all; that they were time-judgments, but derived from an appearance not primarily temporal—derived perhaps from intensive differences in the light at the point of junction of the two halves of the slit. I have myself tried impartially to entertain a supposition of this kind, but after serving long as an observer in the experiment the phenomenon has seemed too me clearly a time-phenomenon; has seemed to be the conscious presence or absence of precise and punctual synchronousness in the luminous slit. There appeared to me no intensive difference between a pair of lights that were exactly simultaneous and a pair in which succession was just discernible; nor is it probable, although still conceivable, that such a difference was there.

Optimal Orders or Directions.—To one other aspect of the observers' judgments attention might be called,—namely to an occasional partiality which the observers had for a particular *order* of succession, a particular *direction* of movement. If we segregate the judgments passed upon a succession actually 'above-below,' and again the judgments passed upon a succession actually 'below-above'; and likewise with motion downward and motion upward, we obtain the values set forth in Tables XI. and XII.

Table XI

Observer B

Segregation of Different Orders and Directions

Time	Succession				Motion				
	Above-below		Below-above		Downward		Upward		
	Successes	Failures	Successes	Failures	Successes	Failures	Successes	Failures	
30σ	6	0	6	0	4	2	4	2	Single presentation—direct vision.
25σ	6	0	6	0	5	1	4	2	
20σ	7	0	6	1	5	2	5	2	
15σ	4	3	6	1	4	3	2	5	
10σ	6	1	4	3	2	5	0	7	
5σ	3	4	3	4	2	5	4	3	
Total.	32	8	31	9	22	18	19	21	
20σ	6	0	5	1	2	4	5	1	Presentation in pairs—direct vision.
15σ	5	1	5	1	5	1	3	3	
10σ	4	2	4	2	4	2	4	2	
5σ	3	3	3	3	0	6	4	2	
Total.	18	6	17	7	11	13	16	8	
30σ	5	1	6	0	5	1	5	1	Presentation in pairs—indirect vision.
25σ	4	2	4	2	5	1	5	1	
20σ	3	3	3	3	2	4	4	2	
15σ	4	2	4	2	2	4	6	0	
Total.	16	8	17	7	14	10	20	4	
Grand total.	66	22	65	23	47	41	55	33	

In Table XII. (Observer S.) it will be noticed that with succession, when the order is above-below the total number of successes is 81, as against 63, when the order is reversed (below-above); and likewise with motion, when direction is downward the successes are 74, as against 58 when the direction is reversed. And this relation that appears in the grand

TABLE XII

OBSERVER S

Segregation of Different Orders and Directions

Time	Succession				Motion				
	Above-below		Below-above		Downward		Upward		
	Successes	Failures	Successes	Failures	Successes	Failures	Successes	Failures	
300σ	5	1	5	1	6	0	6	0	Single presentations — direct vision.
250σ	6	0	5	1	5	1	4	2	
200σ	6	0	6	0	6	0	4	2	
150σ	6	0	3	3	6	0	5	1	
100σ	5	1	0	6	4	2	3	3	
50σ	4	2	2	4	3	3	3	3	
Total.	32	4	21	15	30	6	25	11	
200σ	7	0	7	0	6	1	7	0	Presentation by pairs—direct vision.
150σ	6	1	4	3	7	0	6	1	
100σ	6	1	5	2	5	2	2	5	
50σ	6	1	5	2	4	3	2	5	
Total.	25	3	21	7	22	6	17	11	
300σ	2	0	2	0	2	0	2	0	Presentation by pairs—indirect vision.
250σ	2	0	1	1	2	0	2	0	
200σ	5	1	6	0	6	0	5	1	
150σ	4	2	5	1	5	1	4	2	
100σ	6	0	3	3	5	1	1	5	
50σ	5	1	4	2	2	4	2	4	
Total.	24	4	21	7	22	6	16	12	
Grand total.	81	11	63	29	74	18	58	34	

totals also appears in the sub-totals obtained by each separate method and region of the retina; there is invariably greater success with the order 'above-below,' invariably greater success with the direction downward.

In Table XI. (Observer B.), the partiality is exceedingly faint, if not entirely absent; certainly absent when succession is being offered, for here the total number of right judgments when the order is 'above-below' is almost exactly the same as with the reverse order (66 as against 65) ,and the sub-totals show a like approximation. But with motion there is perhaps a slight partiality for the upward direction (55 successes as against 47 with motion downward),—a partiality, however, so phlegmatic that a mere change of method from double to single presentation is sufficient to reverse it.

Nor is the partiality confined to differences in vertical direction; it appears in experiments with the horizontal direction as well. With Observer S., in whom the difference in result for the different orders of appearance was most pronounced, supplemental experiments were tried in which the stationary slit (*i. e.*, the slit upon the front screen) lay in a horizontal instead of a vertical position, and with the rest of the apparatus so adjusted as to give illuminations of the two halves of the slit in the order 'left-right,' as well as in the order 'right-left.' And also (to answer the question whether the advantage might in any way be due to the fact that the stationary slit was a parallelogram and so permitted the half of the illumination that came from that portion of the radial slit lying nearer the center of the wheel to appear an almost infinitesimally longer period than the illumination from the more distant portion), experiments were performed with two different characters of stationary slit,—in the one case, as hitherto, with sides exactly parallel; in the other case, with sides slightly departing from this arrangement, so that,

Table XIII

Observer S. Direct Vision, Pair Presentation, Horizontal Direction

Succession

Time	Left-right		Right-left		
	Successes	Failures	Successes	Failures	
10σ 5σ	11 6	1 18	12 19	0 5	With parallelogram slit.
	17	19	31	5	
10σ 5σ	9 6	3 6	12 12	0 0	With radial slit.
	15	9	24	0	
Total.	32	28	55	5	

had they been extended, they would have met at the center of the wheel (*i. e.*, a *radial* slit instead of a narrow parallelogram). Thus both halves of the slit gave absolutely equal durations of illumination. In all other respects the procedure was unchanged. Table XIII. shows the outcome of these experiments.

The advantage, which with the vertical slit was for the order 'above-below,' here reappears strong, and for the order 'right-left.' Out of 60 trials with this particular order, but 5 errors were made; whereas for a like number of trials with the opposite order 28 errors were made.

These partialities had already been noticed, as I have said, in the earlier experiments, with the pendulum; and in Table XIV. they are set forth in conjunction with those found in the later experiments.

Table XIV

Unconscious Partialities for Particular Orders and Directions

(*I. e.*, the order or direction in which there was heightened facility, or lowered threshold.)

Observer	In Case of Succession		In Case of Motion	
	Earlier Experiments (Pendulum)	Later Experiments (Wheel)	Earlier Experiments (Pendulum)	Later Experiments (Wheel)
B	above-below (slight)	no preference	upward	upward (slight)
S	above-below	above-below right-left	downward	downward

The suspicion with which I closed the earlier set of experiments, that such partiality was a mere appearance due to some aberration in the apparatus[1] was thus evidently unwarranted. The unequal facility of dealing with the rival orders and rival directions reappears in the later experiments and in the same general sense: the preference for upward motion in the earlier experiments in answered by a preference for the upward motion in the later; the preference for the succession 'above-below' is answered by a preference for this same succession. Only in B., in whom the tendency is weak at best, the partiality grows less in the later experiments, passing away entirely in the case of succession. It is thus an individual matter; for the different observers do not agree in their partialities; and of course it is not to be regarded as a conscious liking. No observer *expressed any preference* for an order or a direction. By 'partiality' is meant, then, no emotional difference, but merely a greater

[1] See p. 272.

ability of perception or of discrimination. It seems to have some similarity (though I can hardly believe that there is any causal connection between the two) to the greater aptitude for detecting a departure from linear continuity when the departure was upon the right, rather than upon the left, of the standard vertical;[1] or to the personal differences in dealing with successive shocks and successive clicks which others have found.[2] In explanation of the partialities appearing in the present experiments, I feel tempted to believe that they may be due to certain obscure habits or tricks of attention. It is probable that, other things equal, some persons are slightly more ready to attend to an object in the upper half of the visual field than in the lower; while with the others the reverse may be true. One whose attention had any such inclination would seem to have the foundation for a partiality like that of the present experiment. It is significant, in this connection, that with S., the advantage which a particular order of succession holds is shared by the corresponding direction of motion. With B., however, this is not true, at the very time when succession in the order 'above-below' had an advantage, the advantage in the region of motion lay with the direction opposed to this, lay namely with motion upward.

V. The Precise Relation between Succession and Motion

Turning now to the main question, which serves as a title to this paper, I believe we shall be more likely to succeed in answering it if we divide and attend to each division separately. (1) Is the consciousness of rapid motion (such as we have had in these experiments) intimately bound up with the consciousness of succession; or are they distinct and even independent mental processes? (2) Are these mental processes, whatever their relation may be, rightly designated as *sensations?* These are separate questions, although the an-

[1] See 'A New Determination of the Minimum Visibile,' etc., Psychological Review, VII., 429; and 'Visible Motion and the Space Threshold,' Psychological Review, IX., 433.

[2] Hamlin, 'On the Least Observable Interval,' etc., *Amer. Journ. of Psychol.*, VI. 564 ff. Drew, 'Attention,' *Amer. Journ. of Psychol.*, VII., 533 ff.

swer to one may involve or determine the answer to the other.

In answer to the preceding of these questions, one may speak first in negatives. I am inclined to believe that the facts brought out by the present experiments, in conjunction with what has been done before, very nearly annul the supposed evidence for the independence of succession and motion; at least so far as the claim for the priority of motion is concerned. For it is now apparent that the detection of motion is nicer than that of discrete succession neither on the spatial side nor yet upon the temporal. Motion cannot be discerned within space-limits too small for discriminating positions; nor within time-limits too narrow to permit the conscious distinction of instants.[1]

On the other hand, a certain claim might now be set up for the priority of discrete sequence. For upon the whole, the threshold when 'succession' was employed was lower than when there was motion. Moreover this meant not simply that an external and objective succession broke up the appearance of simultaneity more readily than did motion, but that the observers were inclined, under the conditions here arranged, and with continued practice, to give their judgment of such non-simultaneity in the form 'sequence' long before they were ready to declare it to be 'motion.' And, indeed, this seems to me in keeping with what we know from a wide variety of experiments: in keeping with the greater ease in deciding the vaguer—the more abstract—feature of an impression; for the decision as to the more definite, the more concrete, characteristics comes later and with more difficulty. Since, in contradiction to Berkeley's principle, we can tell that a pressure has changed, far more readily than we can decide whether it has become heavier or lighter;[2] and since

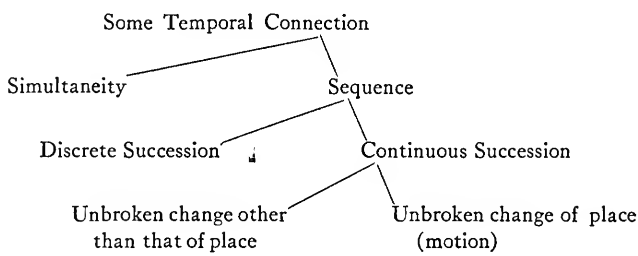
[1] Nor, it might be added, can motion be noticed at an *intensity* too weak for the detection of a motionless light. Contrary to a certain prejudice aroused by the *attention* which moving objects so readily command, a moving light, when attention is full upon it, cannot be seen at so low an intensity of stimulus, as can a stationary light. Here again the supposed sensory-advantage of motion proves ill-claimed. See 'Some Experiments on the Perception of the Movement, Color, and Direction of Lights,' etc., in the *Johns Hopkins Studies in Philosophy and Psychology*, No. 2, pp. 88 f.

[2] See 'Ueber die Wahrnehmung von Druckänderungen,' etc., in Wundt's *Philos. Studien*, XII., 534 ff., 539.

we show a like tendency in many other fields; it would not be surprising if, at bottom, it were easier to decide that simultaneity had in some general way been violated,—that the impression was in some vague form *successive*,—perhaps even without being of a succession distinctly *discrete*—than to decide whether it was succession plus those special and particular marks of *continuity both in space and time* that characterize motion. Succession means merely change; discrete succession means change with some temporal interim or cessation of the process, motion means that the change has no such temporal interim, and furthermore that there is perfect continuity, along with shift, in the space involved. But such an account implies some weakening of the claim to priority on the part of *discrete* succession; for this is almost, though not quite, as advanced a specification of succession as motion is.

The positive side of the answer to the question as to the relation of sequence and motion would therefore seem to me to be this: that motion involves as a constituent element sequence although not discrete sequence; the rather it involves sequence particularized in a way soon to be described; but in any event, that motion is the more special and advanced form of the sequence-consciousness. Yet it is the one to which the mind rushes headlong. For just as in our perception we hasten to fill out the lacunæ of impression, even at the risk of illusion; just as it is, in one sense, far easier to see *as a man* the form and color which a human being offers to the eye and color; and yet this interpretation, while most difficult to suppress, is an advance and addition to the consciousness of mere form and color—so with motion. The rapid sensory sequences which nature offers us in any one region of space are predominantly *continuous* sequences—continuous in both space and time; the flutter of leaves and of grass, the darting of birds and of insects, the quick action of eyes and lips and fingers in men about us. And so the mind becomes accustomed to this manner of interpreting all rapid change; it interprets it instinctively as *motion*. But

this precipitancy of nterpretation should deceive the psychological analyst no more in the one case than in the other. As little as we should now be inclined to say that the perception of a cow is independent of the consciousness of extensity, although most of us see cows in a field more readily than red-extensities there; so little in the end will we be inclined to regard motion, for all its greater readiness to arise in the mind, as independent of sequence or succession. Motion is different from sequence, whether discrete or continuous, but only inasmuch as it is sequence made specific, made more concrete by giving it a peculiar spatial character. The relation of those processes, so far as complexity and dependence is concerned, might be illustrated by the accompanying diagram.



When the mind is not on its guard, it rushes, on the slightest excuse, through all the intermediate stages to a conviction of movement. In that sense, the motion-interpretation is simpler; it is more natural, it is the course in which experience has drilled us. But the mind needs a more *intense* stimulation, it requires an impression whose differential marks are more pronounced, if the mind is to be, not merely more confident, but actually more accurate in detecting the real character of the change presented to it. Practiced observers therefore when confronted with faint changes such as occurred in these experiments unwittingly fall back, sooner or later, on the less commital form of judgment—that of 'succession' merely; passing on to the farther specification,—the judgment that the succession is actually of 'motion'—only when the impression has become more clear. They have become schooled out of their impulsive readiness to take all rapid

visual change (unless there are unmistakable signs to the contrary) as movement.[1]

But the fact that, on the whole, my observers reserved the judgment 'movement' for the more pronounced and definite stimulations, and usually formed only the vaguer judgment 'sequence' or 'succession' or 'some departure from simultaneity,' when the impression was approaching its liminal value,—this does not touch the farther question as to what form of stimulus will best furnish the definiteness of impression that permits even the vaguer judgment to be passed reliably. It would seem from the present experiments that with trained observers discrete succession customarily gives a much greater shock, a less mistakable violation of simultaneity, than does continuous succession (motion). A less time suffices to make discrete succession seem non-simultaneous, than is required to make motion distinct from simultaneity. At times this is not so; the just-perceptible violation of simultaneity comes equally well from either source. But the other relation is perhaps the rule. Indeed, this would seem in keeping with the general intensification and shock and start which interruptions give, and the greater ease and smoothness of continuous change.

To sum up then the answer to the first of our questions, Whether the consciousness of rapid motion is intimately bound up with the consciousness of succession, or is independent; it is this: *that the two are intimately conjoined, the judgment of motion being a farther specification (more complex, and yet easier) of the judgment of sequence. The judgment of motion has a certain priority, in the sense that it is both more complex and also more readily performed*, much as the movement of four fingers of a hand is easier, though more complex, than the movement of one finger alone. *The judgment of succession, however, has its own priority, inasmuch as it is the simpler, and requires a less lasting stimulation to evoke it.*

And the answer to this first question has perhaps already

[1] This naïve readiness is well seen in the almost inevitable impression of 'winking' which a rapid intermission of light produces. The twinkling of stars with its sense of motion is, I doubt not, three fourths motion-illusion due to rapid intensive change, the other fourth the actual shift that the light undergoes by changes of refraction.

suggested the answer I feel bound to give to the second,—the question whether the consciousness of succession or of movement (even when rapid) is to be designated as *sensation*. In dealing here with out consciousness of succession and of movement the expression 'judgment' has often been employed; and yet there is hardly any need of warning that the core-substance of succession or of motion, as they here appear, is not a mere judgment, but has a strong admixture of sensation. We *see* the succession, we *see* the movement, whatever judgments we may pass upon it. But inasmuch as the conscious succession, the conscious movement, involves a certain relating and interpreting (as I have attempted to indicate, especially in regard to motion), *it would seem preferable*—if one were to limit himself to the usual alternatives—*to regard it as a perception, rather than as either a sensation or a judgment.* The sensory impression is here grasped in its simple relations of time, it is here interpreted and felt to belong to a certain familiar type of experience. And just as we speak of the perception of a cow, and the sensation of red; so too in principle (though now the span between the bare impression and its 'form' or 'system' is almost too small for observation) we must distinguish the perception of succession (or the perception of motion) from the sensations which are its ingredients and sign. The term 'sensation,' I take it, designates a sheer abstraction, designates the 'matter' of sensory impressions as distinguished from their 'form.' If then the peculiar meaning of sensation and its distinction from perception (as a consciously formed or organized impression) is to be preserved, it seems best to regard the conscious process with which we have here been so long dealing as a perception or percept, rather than as a sensation.

Yet perhaps the negative side of the argument—that these processes are *not* mere sensations—is perhaps more convincing than the positive side, which urges that they be called 'perceptions.' For the term 'perception' covers an idea that must in time be resurveyed and subdivided. We are sadly in need of a more precise expression here, and were it not for a natural hesitation at new terms, one would feel tempted to

propose that (since no external object is here 'received' or 'taken' through the sense, or the sensation,—which is a prominent element in at least the earlier idea of perception) we designate these simple organizations of sensory 'matter' where no physical 'thing' is caught, as 'infra-percepts,' or 'æsthamorphs.' For there is here the need of marking a stage where sensation has been organized, yet not organized into a perceptive object or thing.

Summary

1. When successive lights are exactly juxtaposed spatially, motion that is just perceived does not occupy a briefer time than mere succession that is just perceived.

2. On the contrary, practiced observers can usually perceive mere succession whose time-limits are less than those of just-noticeable motion.

3. And the judgment of such observers more readily takes a form affirming mere non-simultaneity rather than motion.

4. Under the conditions here arranged, a succession of lights but 5σ apart could be discerned by the writer in 80 per cent. of the trials,—and the other observer also detected succession considerably below the limits hitherto accepted as obtaining in this region.

5. For each person, there is usually an optimal direction of succession or of movement. That is, a succession or movement in the order above-below (or the reverse) may be noticed at a rate at which the succession or motion in the opposite direction is quite imperceptible. This preferred order is not the same for all persons, and in the same individual the preferred order for succession need not be the preferred order for motion.

6. The general relations described above appear to obtain in eccentric as well as in foveal vision, although the absolute values in the two regions may differ.

7. A change of method from that of single presentations to the method of paired presentations generally brings about both a nicer and a more constant recognition of the phenomenon offered.

8. The relation between the consciousness of succession and that of motion is exceedingly intricate. The consciousness of motion is a farther 'specification' of the consciousness of succession, somewhat as the consciousness of increase or decrease of pressure is a farther specification of the consciousness of mere change of pressure. The two orders of perception are certainly distinct, although quite as certainly not independent, since the more specific apprehension always involves the less specific, but not *vice versa.* The seeing of motion is a more elaborate, and yet at the same time more facile mental act, than the seeing of mere succession. The headlong readiness to interpret all neighboring successions as *motions*, to which the world has trained the mind, can however be checked by special and cautious observation, and then the essentially simpler judgment or perception, that of mere succession, is the one that more readily arises.

9. The consciousness of rapid motion or of rapid succession is not a mere sensation, nor is it a mere judgment, nor again is it a perception in the older historic meaning of the term. It is rather a sensation or group of sensations consciously organized, and yet not organized into anything so concrete as a 'thing' or a substantial object. It therefore lies somewhere between sensation and perception, and seems to call for a special designation,—perhaps 'infra-percepts' or 'æsthamorphs.'

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF CALIFORNIA

XVI. Temporal and Accentual Rhythm

BY WARNER BROWN

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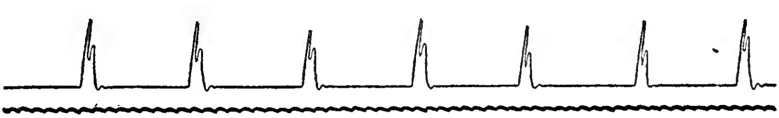
Perhaps the only undisputed characteristic of rhythm is the impression of regularity which it occasions. Some hold that this impression arises from the regular recurrence, in time, of certain features of the rhythmic series; others claim that the regularity resides in the structure of the elements composing the series; but in either case some regularity is admitted. A rhythm lacking regularity in its structure and failing in the regular repetition of its elements would be no rhythm.

I

The mere repetition of a single undifferentiated movement or sound does not constitute a rhythmic series, properly speaking, and yet such a series offers one of the most satisfactory approaches to the more truly rhythmic forms. Movements either with the voice or by tapping, are easily recorded by the kymograph, and may be measured and studied at leisure. On that account a series of movements can be analyzed to better advantage than a series of sounds.



Regular taps

FIG. 1.

Fig. 1 shows a series of regular taps made by the finger. Here regularity is the dominant characteristic. But regularity is seen to be of two distinct sorts. Regularity of

recurrence appears in the duration of the taps and the interval between them, and over against this is regularity in the performance itself, which manifests itself in the force of the movement. In the record the duration is measured horizontally and the force vertically. The degree of regularity

TABLE I

REGULAR TAPS

Showing, for each Subject, the Mean Variation, in per cent., from the average of 20 taps; with regard to the Force and Duration of the taps.

Subject	Force	Duration
1. Mr. Bates	6.22	3.42
2. Mr. Detter	9.02	6.44
3. Mr. Dignan	5.78	2.66
4. Miss Fisher	11.79	2.40
5. Mr. Folte	16.90	4.28
6. Mr. Ham	5.90	3.12
7. Miss Hendee	10.80	5.69
8. Mr. Jackson	8.68	3.81
9. Miss Noteware	6.78	3.03
10. Mr. Robinson	5.94	3.37
11. Miss Replogle	15.50	10.00
12. Mrs. Stanley	13.60	3.31
13. Miss Umphred	11.90	3.50
14. Miss Way	10.95	4.38
15. Mr. Whisman	5.92	4.99
Average	9.71	4.36

is found by computing the mean variation from the average of a number of successive movements.[1] Table I. shows the variation in force and duration of a series of twenty taps with the index finger on the rubber head of a drum connected with a recording tambour. The subjects[2] were requested to tap as regularly as possible in time and force and at a rate of their own choosing. On the whole, averaging the records of the subjects, these taps vary more than twice as much in force as in time. In other words their duration is more than twice as regular as their structural form. For some persons

[1] The measurements of the movements themselves are of no importance in the present connection, and for the sake of clearness only the variations from their average are mentioned. Throughout this paper the mean variation is expressed in per cent. of the quantity which varies.

[2] I am indebted to fifteen of my students in the University of California summer session of 1910 for the records upon which this report is based.

the variation in time is more nearly equal to that in force, but in no case is there less variation in force than in time.

It is evident that this series tends to conserve a regular succession of its elements rather than regularity in their form. But this sacrifice of form to duration occurs in a series which does not profess to have much form, and the further question arises whether the same thing would hold in a truly rhythmic series.

II

Fig. 2 shows a series of taps in rhythm. The subjects were instructed to tap in a natural manner in an iambic rhythm, and the explanation was made that this rhythm has the second beat accented or longer than the first, but no

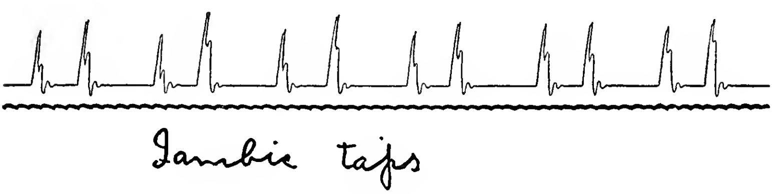


FIG. 2.

indication was given as to the relative importance of the temporal or accentual features. Table II. shows for the fifteen subjects the variation in force and duration of the first (short) and second (long) tap, based on the average of a series of ten pairs of taps. The same table shows the variation in total duration of the rhythmic element (the foot, or sum of two successive taps) and of the total force exerted in the element as measured by the sum of the forces exerted on the two parts of it. This table also gives a measure of the structural constancy of the elements in the series both in force and time. This is shown in the column headed "ratio" by the variation, in the two respects, of the ratio between the two parts of the foot when the long or accented part is divided by the short part.[1]

The more complicated performance demanded by this part of the experiment brings out more individual differences

[1] Again only the variations are considered. The actual ratios will be spoken of later on.

Table II

Iambic Taps

Showing for each Subject the Mean Variation in per cent. from the average of 10 pairs of taps, with regard to the Force and Duration of the taps. The variation is given for the first or Short tap, for the second or Long tap, for the Sum of the two taps, and for the Ratio found by dividing the short into the long tap.

Subject	Short		Long		Sum		Ratio	
	F	D	F	D	F	D	F	D
1	5.71	5.64	1.76	5.94	2.98	3.31	5.32	3.48
2	9.91	5.25	6.64	5.92	4.98	4.19	14.16	8.32
3	9.17	4.55	6.02	2.60	4.02	2.20	11.44	6.35
4	12.71	5.17	9.02	3.56	10.07	3.34	11.83	6.04
5	6.21	6.22	5.83	3.43	3.46	4.31	11.41	5.97
6	16.32	4.48	5.49	2.66	6.06	2.64	19.11	4.62
7	15.00	5.24	7.81	6.24	6.43	4.54	14.50	5.82
8	12.98	6.32	8.92	6.54	4.10	3.79	19.38	9.92
9	5.28	4.59	6.18	5.70	4.11	2.86	6.91	4.68
10	7.81	2.39	5.42	4.90	4.68	2.35	10.75	6.74
11	9.83	4.02	10.56	6.14	6.36	5.32	16.42	4.69
12	14.60	4.44	6.54	2.19	8.31	2.72	12.82	5.17
13	11.40	3.40	7.31	4.18	8.31	3.20	10.80	5.51
14	10.05	6.08	6.00	3.87	3.53	4.57	14.60	7.26
15	9.14	4.48	4.78	4.25	5.04	3.82	10.42	4.04
Average.	10.41	4.82	6.55	4.54	5.50	3.54	12.66	5.91

between the subjects, but in the long run and with very few exceptions the indications of the regular tap series are borne out by the rhythmic series. Whether we consider the short initial stroke, the long accented stroke, or their sum (the whole foot), there is greater variation in force than in time. It should be noted, however, that except for the initial stroke, there is less difference between time and force than in the case of regular taps. Taking the whole foot as a basis for comparion with the regular taps of the first series it is seen that the introduction of rhythmic form has tended to steady the movements both in time and force, but has reduced the variation in force more than in time.

The column headed 'ratio' (Table II.) shows the maintenance of internal structure in the foot or rhythmic unit. On the whole the temporal structure is maintained twice as well as the accentual structure. In neither respect is the internal structure preserved half as well as the uniformity of the rhythmic unit; the ratios vary twice as much as the feet.

III

The third part of the experiment (Fig. 3 and Table III.) reports a series of syllables spoken in iambic tetrameter rhythm and recorded by a tambour in the manner indicated in an earlier paper of the writer's.[1] The subjects were instructed to speak, in a natural rhythm, the written line:

Ta tá, ta tá, ta tá, ta tá;

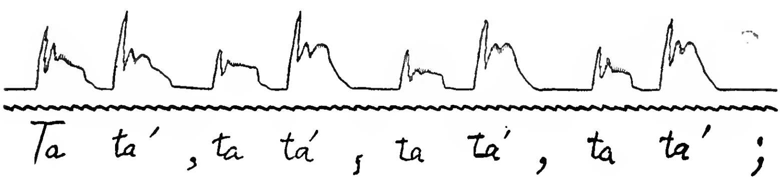


Fig. 3.

Table III

Iambic Syllables

Showing for each subject the Mean Variation in per cent. from the average of ten pairs of syllables from the first foot of the iambic tetrameter line Ta tá, ta tá, ta tá, ta tá; with regard to Force and Duration. The variation is given for the first two syllables separately, for their Sum, and for the Ratio found by dividing the first into the second.

Subject	Ta		tá		Sum		Ratio	
	F	D	F	D	F	D	F	D
1	10.62	4.14	12.56	3.82	11.18	2.76	5.94	9.05
2	9.22	4.29	8.84	5.27	6.23	3.80	9.79	5.45
3	36.00	8.85	18.38	13.80	23.40	9.22	22.00	8.32
4	40.30	10.37	20.30	7.00	22.45	6.33	43.60	11.22
5	15.92	7.34	11.48	2.96	11.82	2.44	15.60	8.58
6	24.70	4.42	30.80	8.08	23.50	3.94	27.40	12.65
7	13.80	11.68	9.14	6.26	4.67	7.10	22.10	8.13
8	13.60	7.04	12.50	5.51	11.80	4.90	12.63	5.76
9	28.00	6.55	19.50	4.77	17.00	4.06	24.70	8.52
10	16.77	7.23	15.43	12.51	15.25	8.22	20.50	15.18
11	15.30	10.90	16.40	8.27	15.50	4.39	9.68	16.65
12	13.55	5.67	26.84	7.00	17.00	5.57	22.10	5.81
13	34.90	21.40	37.20	11.24	25.30	5.39	43.70	28.30
14	17.93	11.00	16.82	6.81	14.50	5.97	22.40	8.23
15	25.80	6.63	11.53	4.13	15.43	4.41	23.50	10.76
Average.	21.09	8.50	17.81	7.16	15.67	5.23	21.71	10.84

like a line of verse, and then to repeat the same line as another verse and so on until stopped. The first foot of each of ten

[1] 'Time in English Verse Rhythm,' *Archives of Psychol.*, No. 10, 1908.

verses was measured for the record. The force is the height of the consonant, and the duration is the time from the beginning of one consonant to the beginning of the next. Here all the conditions, rhythmic form, motor performance, and experimental conditions for making the record, were more complicated than in the case of taps. The difficulty of securing an adequate record of the force of the vocal utterance is a particularly serious source of error,[1] and yet when we remember that the syllables to be measured are all made up of the same letters or sounds the data may be considered sufficiently reliable for our purposes. As compared with the iambic taps there is a very large increase in variability in all directions, and while the larger variation in force may be attributable to instrumental difficulties, the loss of control in time is beyond question. This greater variability indicates that the greater elaborateness of the rhythm or the change in the motor mechanism, or the experimental embarrassments (which were not grave) singly, or together, interfered with the rhythm in respect both to internal structure and to the relation of the units to one another.

Yet apart from greater irregularity, the spoken iambs do not differ materially from those that were tapped, when we consider that the blame for the relatively greater variability in force can properly be laid to instrumental difficulties. In Table II. the difference between time and force was more noticeable in the ratio column than in any other, but in the present case the ratios differ in force only twice as much as in duration, while in the other columns the force varies nearly three times as much as the duration. This is not to be taken as improvement in the relative regularity of structure as regards force, but again as a peculiarity of the recording device. This device might well record the relative emphasis, *i. e.*, the ratio of force, within the foot, while it would not report correctly the emphasis in different feet, or over any considerable length of time.

[1] See 'Time in Eng. Verse Rhythm,' p. 22; and compare Bourdon in *L'année psychologique*, IV., 1898, p. 370.

IV

In the fourth part of the experiment the subjects were instructed to recite into the recording apparatus in a natural but forceful manner the nursery verses:

Pease porridge hot,
Pease porridge cold,
Pease porridge in the pot nine days old.

Table IV

PEASE PORRIDGE

Showing for each Subject the Mean Variation in per cent. from an average of 10 measurements of the words Pease porridge cold; with regard to Force and Duration. The Variation is shown for each word separately, for the Sum of the three words, and for the Ratio found by dividing the first word into the second.

Subject	Pease		Porridge		Cold		Sum		Ratio	
	F	D	F	D	F	D	F	D	F	D
1	15.28	6.06	10.86	4.02	14.29	7.74	9.75	3.87	14.68	3.08
2	23.20	11.55	29.60	7.11	22.40	6.01	21.20	5.71	32.80	14.05
3	12.40	8.87	13.00	8.28	10.17	6.80	8.07	3.85	22.05	14.51
4	[1]	[1]	[1]	[1]	[1]	[1]	[1]	[1]	[1]	[1]
5	[1]	10.90	[1]	8.30	[1]	6.37	[1]	6.74	[1]	12.77
6	25.15	9.77	37.10	4.11	31.85	7.76	22.75	5.54	46.70	8.01
7	26.60	7.46	15.80	3.66	15.80	3.45	16.30	3.16	22.80	7.25
8	17.15	13.50	17.90	7.26	18.00	5.54	14.53	4.87	15.20	17.70
9	16.00	6.00	16.60	3.84	31.70	8.00	17.20	3.84	15.40	4.03
10	17.88	6.50	16.67	6.24	40.20	8.04	16.93	3.56	25.60	9.85
11	20.20	8.98	22.20	11.18	25.80	3.96	16.60	3.36	23.00	19.30
12	11.90	4.72	10.70	1.98	32.40	4.04	7.71	1.65	16.65	4.66
13	25.70	7.05	28.00	5.61	32.40	8.92	15.40	4.72	43.00	10.60
14	9.58	6.02	13.40	6.87	31.20	12.40	11.15	6.91	19.25	9.64
15	24.80	6.19	17.60	3.44	25.70	6.81	13.00	4.40	38.60	6.78
Av.	18.91	8.11	19.19	5.86	25.54	6.84	14.66	4.44	25.82	10.16

This was repeated a number of times. The record, given in Fig. 4 and Table IV., shows the variation in force (height

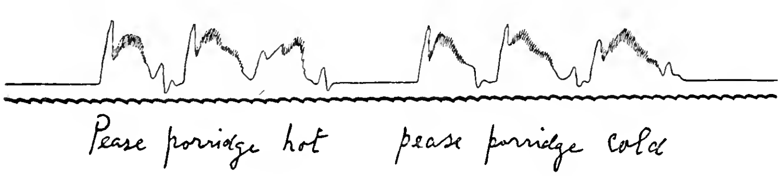


FIG. 4.

of the initial consonants p, p and c) in the phrase 'Pease porridge cold,' and in time from the beginning of pease to the

[1] No legible record.

beginning of porridge, from the beginning of porridge to the beginning of cold, and in the duration of the word cold to the end of the final consonant. It also shows the variations in time and force for the whole phrase of three words and in the ratios for the first and second words of the phrase. The variations are based on the average of ten records except that in three cases there were only eight available records. As elsewhere the variations are expressed in per cent. of the quantities involved.

With regard to either the time or force, the variations in this series are about as large for the separate parts of the foot as in the iambic syllable (ta tá) series. The variation in force exceeds the variation in time in about the same proportion as in that series. But the variations for the foot as a whole fall below those for the iambic syllables. In duration these feet are about as regular as the regular taps of the first series. The ratios present a different aspect, for while in duration they are slightly more regular than the iambic syllables, they are less regular in force. In comparison with the series of iambic syllables the 'Pease porridge' series is more regular in recurrence in regard to both the time and force of the total foot. The 'Pease porridge' series has also a more regular internal structure (ratio) in regard to time but not in regard to force. Loss of control over the relative force of the movements is very evident in even a superficial inspection of the 'Pease porridge' records. The relative force of the two p's often undergoes a complete reversal from verse to verse. The rhythm of this verse is of course irregular or even amorphic but it is of a very distinctly temporal type, giving, all the disturbing factors being considered, very great regularity in the matter of recurrence, together with a relatively high degree of regularity in internal structure so far as the time relations are concerned.

With these concrete examples from which to start we are now in a position to discuss with more understanding the temporal and accentual features in any rhythm. The first point to be observed is that all recurrence is a temporal

matter. A rhythm is temporal in so far as there is any regular return of similar features. But at the same time such a rhythm will also be accentual since there must always be points of emphasis whose return can be marked. At this stage of the discussion the question to be answered is: Which is fundamental to the rhythm; the uniform time of recurrence, or the uniform character of the thing that recurs? Assuming a reliable method of measuring both the thing and its rate of recurrence I propose the variability as a test in this question. If the movements or sounds vary in intensity more than in duration or more than the interval separating them, I submit that the rhythm is primarily temporal. No very extended argument seems to be required in support of this view, for regularity is essential to rhythm, and if the regularity is predominantly in the time relations the rhythm may be presumed to have its seat there also. Accent may be a necessary feature but it is not the distinctive feature in the rhythms examined for this study.

But we can not stop the discussion on the level of mere recurrence; that would be to stop with the whole feet, ignoring their parts. We can not consider the mere intensity and duration of the item which recurs; we must also consider its individual make-up or structural character. This puts us on the second level, that of the structure of the elements composing the rhythm. Here we find again intensities and durations, or to speak more accurately, relations of intensity and duration. May it not be that we shall find in one or the other of these relations something that will present greater regularity than the mere recurrence of the elements? If so we may look here for the essence of the rhythm.

On the surface of the returns the ratios which represent these relations appear to be more variable than the periods of recurrence. But as the writer has stated before,[1] this view although probably correct can not be taken as final, because the ratios are apparently incommensurable with the actual durations. But we can face this difficulty fairly, and still say that on this second level of structural form, consider-

[1] 'Time in English Verse Rhythm,' p. 67.

ing the ratios independently, it is in the temporal structure and not in the accentual structure that we find the greater regularity. If the fundamental regularity does reside in the arrangement of the parts within the units rather than in the recurrence of these units, it is still to be sought in the arrangement of the parts in time. In any event the predominant regularity on either level is a temporal regularity.

V

A somewhat different method of attack leads to the same conclusion. If, in Table V., we consider the actual ratios which represent the structure of the elements, we find that the ratios for force of movement are different from the ratios

Table V

Ratios

Showing for each Subject the average absolute amount of the Ratio found by dividing the first tap or syllable into the second; with regard to Force and Duration.

Subject.	Iambic Taps Cf. Table II		Ta tá Cf. Table III		Pease porridge Cf. Table IV	
	F	D	F	D	F	D
1	1.22	1.78	1.68	2.18	1.09	1.10
2	1.29	2.45	1.23	1.96	.86	1.14
3	1.38	1.71	1.89	2.56	1.14	1.33
4	1.57	2.52	2.46	2.56	——	——
5	1.28	1.73	2.24	2.93	——	1.14
6	1.87	.95	1.55	1.71	1.02	1.14
7	1.87	1.61	1.92	2.64	.98	1.09
8	1.58	1.19	.94	1.91	.98	1.12
9	1.09	1.79	2.44	3.06	.99	1.02
10	1.24	1.07	1.53	2.47	.55	.94
11	1.62	2.94	1.05	2.13	.93	1.16
12	1.27	2.10	1.14	2.00	.72	1.05
13	1.64	1.82	2.82	2.84	1.16	1.08
14	1.17	1.96	1.77	2.22	.67	1.13
15	1.52	2.47	2.11	3.31	.86	1.49
Average.	1.44	1.87	1.78	2.43	.92	1.14

for duration of movement. In both of the iambic series the time-ratios are larger than the force-ratios. That is to say, the typical iambic structure with the second part outweighing the first part is more adequately or fully carried out in time than in stress. If the essence of the rhythm is on the second level, in the arrangement of the parts within the rhythmic

unit, then again the arrangement in time satisfies the requirements more fully.

No inferences can be drawn from the absolute ratios of the 'Pease porridge' records because there is no evidence that this was meant by the speakers to be an iambic ryhthm. It has some of the ear-marks of a trochaic rhythm[1] and in that case we do not know whether a larger or smaller ratio stands for the typical rhythm. In fact the absolute ratio fails when applied to verse rhythms because of the fact here illustrated that the time-ratio is determined in such cases by the necessary time of uttering the words. The second word in this verse appears to be the second element in a trochaic foot; it sounds so to the ear and its initial consonant shows in the record less stress, on the average, than the initial consonant of the first word; and yet it takes longer to say the longer and more complex second word. In verse there are no typical time-ratios and the force-ratios are not easily measured. With this exception the evidence from the absolute ratio furnishes a valuable check upon the evidence from the relative variabilities.

These results are presented with the hope of arousing interest in what appears to me to be a promising method for the analysis of some of the fundamental questions regarding the basis of rhythmic actions and impressions. The results themselves, so far as they go, indicate as I think, that the time aspects are fundamental and that the accentual features while necessary are not at the root of the phenomena.

[1] *Op. cit.*, p. 55.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF CALIFORNIA

XVII. Some Preferences by Boys and Girls as Shown in their Choice of Words

BY M. I. STOCKTON

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XVII. SOME PREFERENCES BY BOYS AND GIRLS AS SHOWN IN THEIR CHOICE OF WORDS

BY M. I. STOCKTON

The broader purpose of the experiments reported in this paper was to obtain light upon possible differences in the affective life of children at different ages, and especially upon any differences between boys and girls. The present experiments were limited, however, to a study of the difference between boys and girls as shown in their choice between time and space words; between words expressing activity and those expressing passivity; between words relating to dress and those relating to food; between verbs and adjectives.

These experiments were suggested by the conclusions stated in Mrs. Manchester's paper on 'Unreflective Ideas of Men and Women.'[1] The question in her study was whether there is any difference between college men and women in their unreflective ideas. From the results of her experiment she drew the following general conclusions:

1. The surface ideas of men and women pertain to objects which are familiar and interesting.
2. The dynamic aspect of objects is more attractive to men, while the static or completed aspect appeals more to women.
3. Time as a factor enters more largely into the surface ideas of men; space is more often a prominent feature of the surface ideas of women.
4. Men make a greater use of abstract terms, while women show a preference for concrete and descriptive words.

The present experiments were carried out in all the grades, above the low second, of the Bay Grammar School, Oakland, Cal. The accompanying table (Table A) gives the age, number and distribution of the pupils participating.

[1] PSYCHOLOGICAL REVIEW, Vol. 12 (1905), p. 50.

Method.—It was thought that if a pair of words of equal difficulty and familiarity were offered, one of which was to be selected, although several motives might affect the choice, yet perhaps there might, after elimination and check, be discovered some clue to the attractiveness of the ideas themselves. Four lists of twenty pairs of words were selected. In the first list, each pair was composed of a time word and a space word; in the second list, of a word denoting activity and one denoting passivity; in the third list, of one referring to dress and one referring to food; in the fourth list, of a verb and an adjective.

TABLE A

Grade	Average Number of Pupils Participating in the Four Sets of Experiments		Average Age	
	Boys	Girls	Boys	Girls
B2	21	19	9.3	8.6
A3	20	17	10.1	9.3
B3	14	17	10.9	9.3
A4	21	17	10.9	11
B4	9	8	12.4	11.7
A5	6	17	11.3	12.1
B5	11	15	13.9	12.1
A6	18	12	13.3	12.9
B6	4	8	13.6	14
A7	14	13	13.6	12.9
B7	6	9	14.5	14
A8	4	6	14.6	14.3
B8	2	4	15.6	15.3

Each pair of words was written, one directly under the other, on a large card which every pupil could see distinctly when the teacher showed it to the class. The cards were numbered so that the words could be presented in order. The words were so written that if a time word was first on one card, a space word was first on the next following; a time word on the third and so on. In this way, any preference due to the position of the word on the card would be offset. Such an order was carefully followed in each of the four lists, as will be seen by glancing at the words on pages 349 and 350.

Before the cards were presented, paper was distributed to the pupils. They were told to write only one of the two words that appeared on each card. No further information concerning the work was given. With the exception of the

third set of papers from the eighth grade, the words were written by the pupils of the twelve different classes at the the same hour on each of the four days.

The lists containing time and space words and words denoting activity and passivity were given on the first day. These have been designated Set I. Set II. was given five weeks later and contained the list of words pertaining to dress and food and the list of verbs and adjectives. Set III. was composed of the words of Set I, but the order of the words of each pair was reversed. In this way, a time word that had been first of a pair in Set I., was second of the same pair in Set III. By this double check—that is by alternating within the set itself the class of word that appeared first on each card of the twenty, and by reversing in Set III. the position of each word in its pair as it appeared in Set I. — any influence which might be due to preference for a word merely because it was first or second in its pair would probably in the long run

List 1 Time and Space Words		List 2 Words Denoting Activity and Passivity	
mile	little	run	romp
year	month	sit	hush
always	soon	rest	calm
under	tall	busy	move
big	down	swim	lively
now	daily	still	softly
later	tomorrow	sleep	silent
small	thickness	work	hurry
where	width	talking	chatter
when	quick	listen	slumber
hour	seldom	quiet	patient
high	narrow	throw	speaking
inch	above	dig	climb
slow	fast	wait	float
early	yesterday	idle	grow
large	outside	jump	push
below	broad	play	day
today	until	ride	night
then	time	evening	standing
there	space	morning	chasing

LIST 3

Words Relating to Dress and Food

dress	collar
grapes	cheese
nut	banana
cap	ribbon
hat	velvet
pie	apple
berries	supper
gloves	cloak
coat	silk
cake	food
bread	beef
skirt	lace
vest	tailor
meat	turkey
fruit	pudding
shoes	necktie
stockings	button
potatoes	carrot
butter	dinner
woolen	shawl

LIST 4

Verbs and Adjectives

build	sing
little	poor
fresh	old
threw	buy
give	sailed
hot	happy
large	green
grow	lived
tell	sold
good	glad
rich	long
pick	make
looked	run
short	red
sweet	kind
think	jump
follow	slept
sunny	small
fine	merry
eat	break

be compensated. The third set was presented two weeks after the second set. Set IV. contained the words of Set II. in reverse order and was given three weeks later than Set III.

The pupils of the A second grade because of insufficient power of writing were unsuited to the experiment; so the B second grade is the youngest class employed, and is referred to simply as the second grade. There were so few pupils in the eighth grade that the results of the two divisions together have been given as the eighth grade. The work of the A fourth grade was interrupted on two mornings, so there is only one set of papers for the list of verbs and adjectives and for the list containing words denoting activity and passivity.

Many difficulties beset one in preparing the lists of words. There were serious limitations placed upon selection by the difference in ability of the various classes. It was necessary that the two words of a pair, fairly intelligible and within the vocabulary of the pupils, should be of the same length

and of the same degree of difficulty in writing. Although much time was spent in preparing the lists, it is impossible to feel that they are entirely satisfactory or beyond criticism.

The results are arranged in Tables I. to IV. which show the percentage of each class of words selected by the boys and girls respectively in the different grades and also the percentage of first and second words selected in the same lists. The percentages are given for each set of papers obtained from each of the four lists of words and also the average percentage of the two sets.

In Table V., the total number of each class of words selected by the twelve grades is given with the corresponding number of first and second words chosen. The percentage selected is also shown. The more significant figures are those given as percentages of the different classes of words selected. The total number of words is of less value since the proportion of boys and of girls varies in the different grades.

Plates A to D inclusive give in different form the same results as are given in Tables I. to IV. The ordinates represent the percentages of preference; the abscissæ represent in order the different school grades. The average value of each curve for all twelve grades is also shown in each table by the horizontal lines; *e. g.*, the horizontal line composed of a dot and a dash in Plate A represents the average selection of time words for the twelve grades for the boys,—having a value of 53.9 per cent. Plate E shows the average percentage of the different classes of words and of first and second words for the twelve grades combined.

Let us consider, first, what conclusions one is warranted in drawing with regard to the more limited problem of this experiment; and later the relation of these results to the larger field will be of interest.

The averages of the two sets in Tables I.–IV. are the significant figures; for any apparent preference due merely to the order of the words is probably eliminated by the alternation and reversal of words explained on page 348. From Table I. and its graphic presentation in Plates A and E, one

Table I

Percentage of Words Chosen

Character of the Word	Boys				Girls			
	Time	Space	First	Second	Time	Space	First	Second
Second Grade.								
First set	54.2	45.8	43.9	56.1	48.6	51.4	50.5	49.5
Third set	49.6	50.4	42.9	57.1	50.4	49.6	62.1	37.9
Average	51.8	48.2	43.4	56.6	49.5	50.5	56.3	43.7
A Third Grade.								
First set	53.7	46.3	56.3	43.7	53.1	46.9	62.7	37.3
Third set	57.4	42.6	60.4	39.6	53.3	46.7	64.2	35.8
Average	55.5	44.5	58.4	41.6	53.2	46.8	63.4	36.6
B Third Grade.								
First set	55.4	44.6	37.6	62.4	52.5	47.5	37.2	62.8
Third set	51.0	49.0	69.2	30.8	51.0	49.0	69.2	30.8
Average	53.2	46.8	53.4	46.6	51.7	48.3	53.2	46.8
A Fourth Grade.								
First set	56.2	43.8	50.7	49.3	53.0	47.0	51.0	49.0
Third set	54.6	45.4	58.9	41.1	43.3	56.7	53.9	46.1
Average	55.4	44.6	54.8	45.2	48.1	51.9	52.4	47.6
B Fourth Grade.								
First set	50.0	50.0	55.5	44.5	51.9	48.1	33.1	66.9
Third set	50.2	49.8	72.3	27.7	53.4	46.6	46.6	53.4
Average	50.1	49.9	63.9	36.1	52.6	47.4	39.8	60.2
A Fifth Grade.								
First set	54.2	45.8	34.2	65.8	50.9	49.1	42.9	57.1
Third set	50.7	49.3	62.1	37.9	52.8	47.2	47.2	42.8
Average	52.5	47.5	48.2	51.8	51.8	48.2	50.1	49.9
B Fifth Grade.								
First set	49.0	51.0	40.0	60.0	55.7	44.3	44.3	55.7
Third set	56.0	44.0	61.4	38.6	55.4	44.6	59.3	40.7
Average	52.5	47.5	50.7	49.3	55.5	44.5	51.8	48.2
A Sixth Grade.								
First set	54.7	45.3	52.2	47.8	48.8	51.2	51.2	48.8
Third set	51.4	48.6	63.9	36.1	53.9	46.1	70.7	29.3
Average	53.0	47.0	58.1	41.9	51.3	48.7	60.9	39.1
B Sixth Grade.								
First set	48.8	51.2	53.7	46.3	50.7	49.3	67.8	32.2
Third set	55.0	45.0	55.0	45.0	50.0	50.0	74.3	25.7
Average	51.9	48.1	54.4	45.6	50.3	49.7	71.1	28.9
A Seventh Grade.								
First set	56.6	43.4	44.6	55.4	53.7	46.3	48.9	51.1
Third set	58.8	41.2	64.3	35.7	55.9	44.1	67.2	32.8
Average	57.7	42.3	54.5	45.5	54.8	45.2	58.1	41.9
B Seventh Grade.								
First set	58.8	41.2	43.8	56.2	55.7	44.3	52.2	47.8
Third set	47.9	52.1	68.9	31.1	69.5	30.5	58.7	41.2
Average	53.3	46.7	56.4	43.6	62.6	37.4	55.5	44.5
Eighth Grade.								
First set	60.8	39.2	53.3	46.7	57.9	42.1	36.7	63.3
Third set	51.3	48.7	68.8	31.2	49.1	50.9	80.0	20.0
Average	56.0	44.0	61.1	38.9	53.5	46.5	58.3	41.7

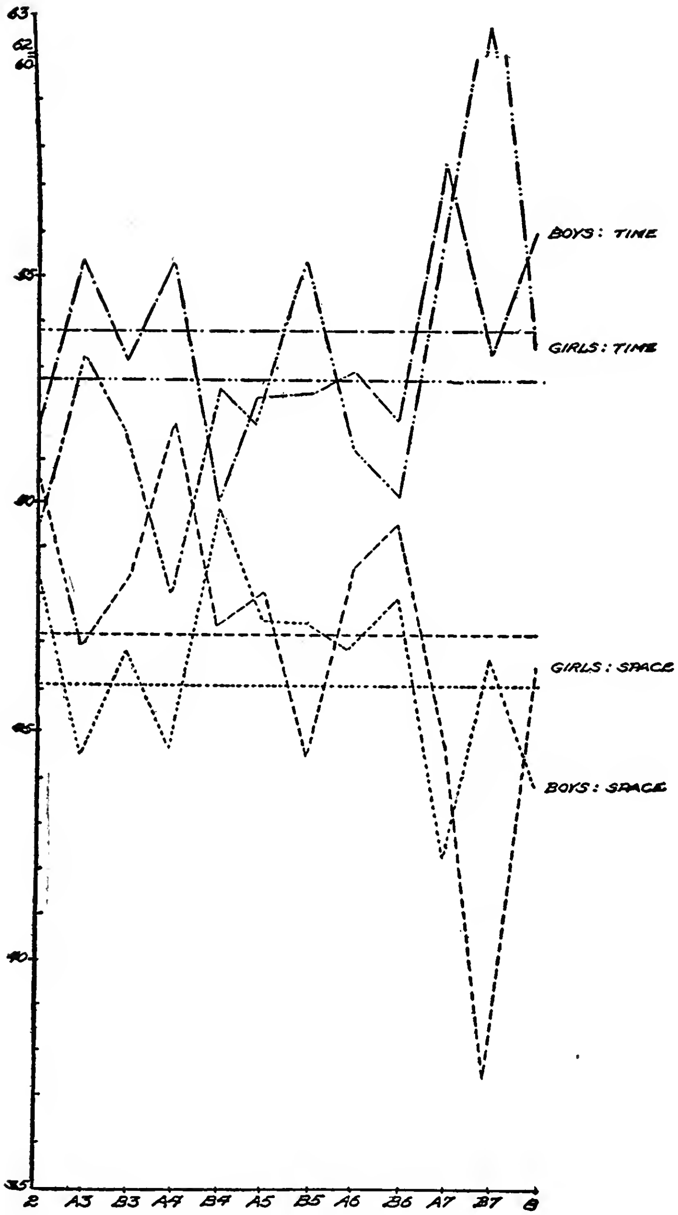

PLATE A. Choice between time-words and space-words, selected by boys and girls respectively. The ordinates indicate in per cent. the amount of preference shown for each of the two groups of words; the abscissæ, the school-grades. (From Table I.)

Table II

Percentage of Words Chosen

Character of the Word	Boys				Girls			
	Activity	Passivity	First	Second	Activity	Passivity	First	Second
Second Grade.								
First set	49.5	50.5	45.4	54.6	53.5	46.5	48.5	51.5
Third set	46.8	53.2	41.6	58.4	53.2	46.8	54.8	45.2
Average	48.2	51.8	43.5	56.5	53.4	46.6	51.6	48.4
A Third Grade.								
First set	51.6	48.4	60.5	39.5	51.1	48.9	57.1	42.9
Third set	51.8	48.2	51.5	48.5	50.0	50.0	63.4	36.6
Average	51.7	48.3	56.0	44.0	50.5	49.5	60.3	39.7
B Third Grade.								
First set	54.5	45.5	44.7	55.3	54.2	45.8	45.1	54.9
Third set	41.6	58.4	63.9	36.1	49.1	50.9	58.3	41.7
Average	48.0	52.0	54.3	45.7	51.6	48.4	51.7	48.3
A Fourth Grade.								
First set	50.9	49.1	52.9	47.1	46.0	54.0	51.0	49.0
B Fourth Grade.								
First set	48.3	51.7	60.6	39.4	43.1	56.9	39.4	60.6
Third set	51.3	48.7	67.4	32.6	36.1	63.9	57.9	42.1
Average	49.8	50.2	64.0	36.0	39.6	60.4	48.6	51.4
A Fifth Grade.								
First set	45.4	54.6	36.2	63.8	50.1	49.9	51.2	48.8
Third set	53.9	46.1	61.8	38.2	48.5	51.5	51.5	48.5
Average	49.6	50.4	49.0	51.0	49.3	50.7	51.4	48.6
B Fifth Grade.								
First set	49.5	50.5	46.5	53.5	48.1	51.9	47.5	52.5
Third set	58.9	41.1	50.3	49.7	49.2	50.8	53.1	46.9
Average	54.2	45.8	48.4	51.6	48.6	51.4	50.3	49.7
A Sixth Grade.								
First set	54.5	45.5	47.5	52.5	51.2	48.8	53.5	46.5
Third set	50.6	49.4	70.1	29.9	48.8	51.2	42.2	57.8
Average	52.5	47.5	58.8	41.2	50.0	50.0	47.8	52.2
B Sixth Grade.								
First set	56.2	43.8	71.2	28.8	47.1	52.9	72.8	27.2
Third set	54.5	45.5	48.5	51.5	43.6	56.4	73.6	26.4
Average	55.4	44.6	59.8	40.2	45.4	54.6	73.2	26.8
A Seventh Grade								
First set	51.7	48.3	52.3	47.7	50.4	49.6	56.1	43.9
Third set	57.2	42.8	59.7	40.3	45.6	54.4	56.9	43.1
Average	54.5	45.5	56.0	44.0	48.0	52.0	56.5	43.5
B Seventh Grade.								
First set	47.5	52.5	52.5	47.5	44.3	55.7	52.8	47.2
Third set	49.1	50.9	70.2	29.8	43.5	56.5	57.1	42.9
Average	48.3	51.7	61.4	38.6	43.9	56.1	54.9	45.1
Eighth Grade.								
First set	52.1	47.9	49.6	50.4	48.3	51.7	39.2	60.8
Third set	46.4	53.6	73.7	26.3	52.2	47.8	84.1	15.9
Average	49.3	50.7	61.6	38.4	50.3	49.7	61.6	38.4

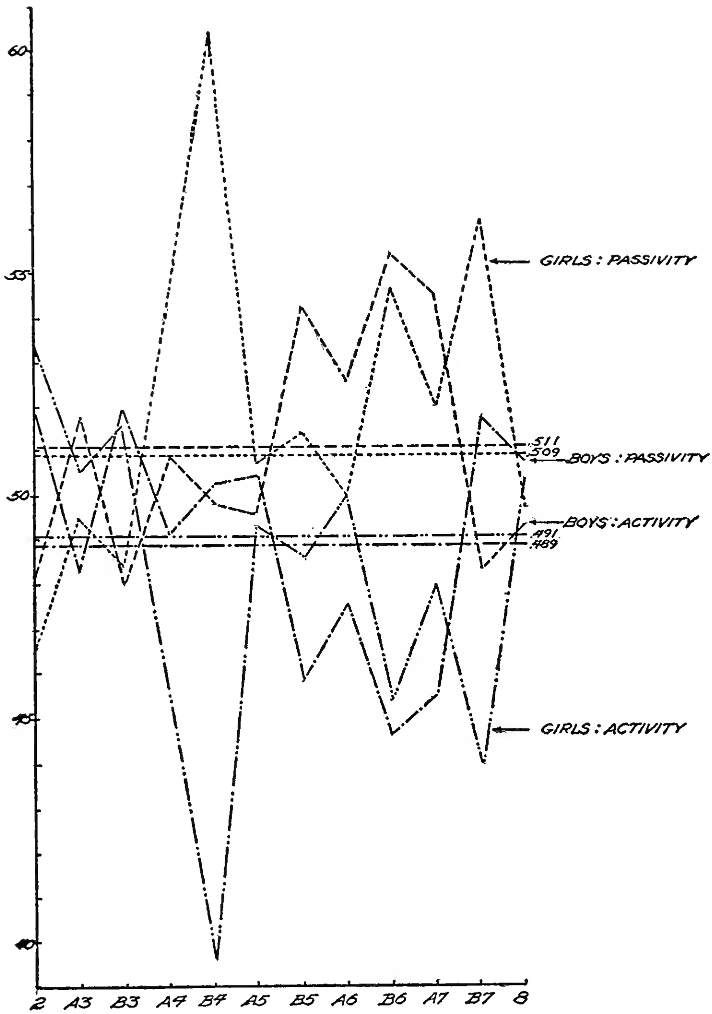

PLATE B. Choice between words denoting activity and passivity by boys and girls respectively. The ordinates indicate in per cent. the preference shown for each of the two groups of words, the abscissæ, the school-grades. (From Table II.)

may conclude that a noticeable predominance of time interest is shown by both boys and girls. There is a slightly greater predominance of time interest among the boys than among the girls. In these same sets of time and space words, there is a predominance of first words in ten grades among the boys and in eleven grades among the girls.

Table III

Percentage of Words Chosen

Character of Word Chosen	Boys				Girls			
	Dress	Food	First	Second	Dress	Food	First	Second
Second Grade.								
Second set	50.7	49.3	60.3	39.7	50.8	49.2	56.1	43.9
Fourth set	48.5	51.5	39.6	60.4	37.3	62.7	51.7	48.3
Average	49.6	50.4	49.9	50.1	44.1	55.9	53.9	46.1
A Third Grade.								
Second set	49.9	50.1	41.3	58.7	58.1	41.9	52.7	47.3
Fourth set	49.7	50.3	72.6	27.4	50.8	49.2	72.1	27.9
Average	49.8	50.2	56.9	43.1	54.5	45.5	62.4	37.6
B Third Grade.								
Second set	41.1	58.9	53.3	46.7	41.1	58.9	53.5	46.5
Fourth set	55.4	44.6	64.8	35.2	44.5	55.5	57.4	42.6
Average	48.3	51.7	59.1	40.9	42.8	57.2	55.5	44.5
A Fourth Grade.								
Second set	40.0	60.0	59.0	41.0	39.6	60.4	46.4	53.6
Fourth set	42.9	57.1	57.1	42.9	43.1	56.9	57.3	42.7
Average	41.5	58.5	58.1	41.9	41.4	58.6	51.8	48.2
B Fourth Grade.								
Second set	49.7	50.3	61.3	38.7	45.0	55.0	42.9	57.1
Fourth set	45.5	54.5	57.1	42.9	47.6	52.4	59.5	40.5
Average	47.6	52.4	59.2	40.8	46.3	53.7	51.2	48.8
A Fifth Grade.								
Second set	42.2	57.8	51.9	48.1	49.1	50.9	52.3	47.7
Fourth set	41.7	58.3	44.8	55.2	49.4	50.6	57.9	42.1
Average	41.9	58.1	48.4	51.6	49.3	50.7	55.1	44.9
B Fifth Grade.								
Second set	56.4	43.6	66.1	33.9	44.8	55.2	51.3	48.7
Fourth set	31.5	68.5	53.2	46.8	44.1	55.9	58.1	41.9
Average	43.9	56.1	59.6	40.4	44.5	55.5	54.7	45.3
A Sixth Grade.								
Second set	38.5	61.5	63.6	36.4	55.9	44.1	67.9	32.1
Fourth set	28.9	71.1	66.1	33.9	52.9	47.1	67.1	32.9
Average	33.7	66.3	64.8	35.2	54.4	45.6	67.5	32.5
B Sixth Grade.								
Second set	30.0	70.0	57.5	42.5	44.1	55.9	77.4	22.6
Fourth set	20.0	80.0	65.0	35.0	45.0	55.0	67.5	32.5
Average	25.0	75.0	61.3	38.7	44.5	55.5	72.5	27.5
A Seventh Grade.								
Second set	37.6	62.4	56.2	43.8	37.7	62.3	45.8	54.2
Fourth set	18.6	81.4	53.6	46.4	24.1	75.9	55.5	44.5
Average	28.1	71.9	54.9	45.1	30.9	69.1	50.6	49.4
B Seventh Grade.								
Second set	52.5	47.5	50.8	49.2	53.6	46.4	50.3	49.7
Fourth set	34.2	65.8	42.5	57.5	40.6	59.4	57.3	42.7
Average	43.4	56.6	46.6	53.4	47.1	52.9	53.8	46.2
Eighth Grade.								
Second set	45.8	54.2	57.5	42.5	47.1	52.9	53.8	46.2
Fourth set	38.8	61.2	71.2	28.8	45.9	54.1	58.7	41.3
Average	42.3	57.7	64.4	35.6	46.5	53.5	56.3	43.7

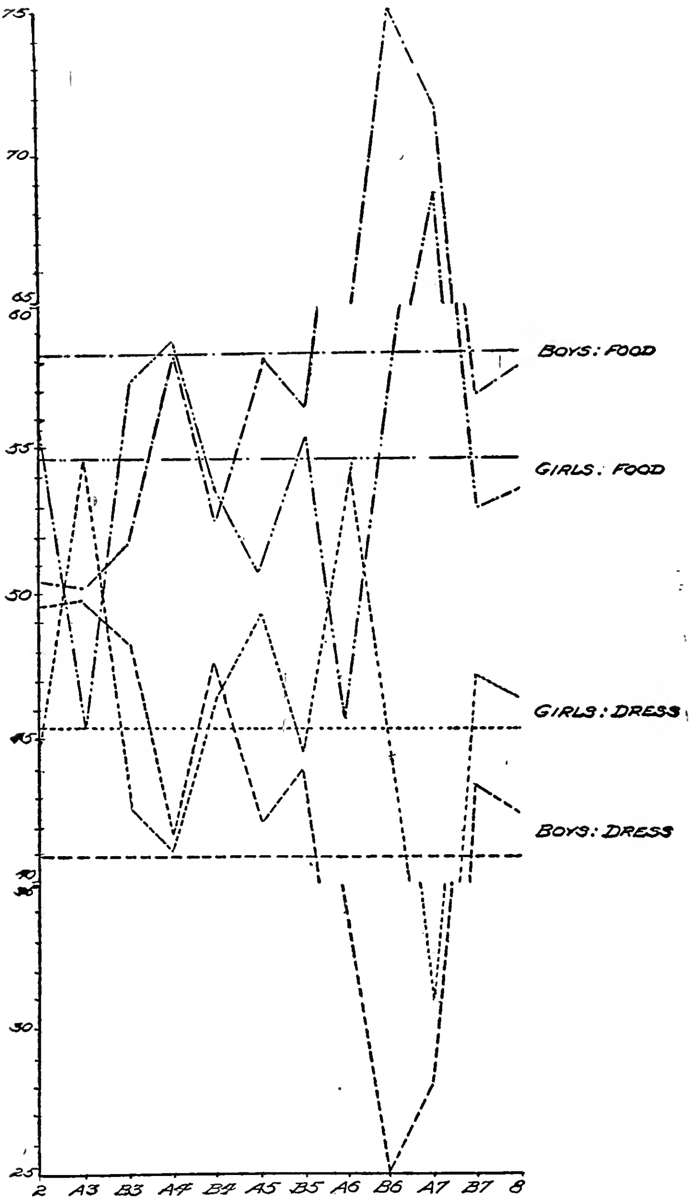

PLATE C. Choice between words denoting food and dress, by boys and girls respectively. The ordinates indicate in per cent. the amount of preference shown for each of the two groups of words; the abscissæ, the school-grades. (From Table III.)

Table IV

PERCENTAGE OF WORDS CHOSEN

Character of Word Chosen	Boys				Girls			
	Verbs	Adjectives	First	Second	Verbs	Adjectives	First	Second
Second Grade.								
Second set	45.8	54.2	58.7	41.3	46.8	53.2	59.5	40.5
Fourth set	45.3	54.7	40.8	59.3	36.4	63.6	54.4	45.6
Average	45.5	54.5	49.7	50.3	41.6	58.4	56.9	43.1
A Third Grade.								
Second set	43.1	56.9	55.4	44.6	43.4	56.6	62.4	37.6
Fourth set	45.6	54.4	69.7	30.3	46.6	53.4	80.1	19.9
Average	44.4	55.6	62.5	37.5	45.0	55.0	71.3	28.7
B Third Grade.								
Second set	43.7	56.3	66.9	33.1	42.2	57.8	56.9	43.1
Fourth set	46.2	53.8	69.6	30.4	42.1	57.9	64.1	35.9
Average	44.9	55.1	68.3	31.7	42.2	57.8	60.5	39.5
A Fourth Grade.								
Fourth set	44.7	55.3	51.9	48.1	43.3	56.7	56.3	43.7
B Fourth Grade.								
Second set	44.0	56.0	61.0	39.0	42.5	57.5	49.1	50.9
Fourth set	43.8	56.2	67.2	32.8	38.5	61.5	63.2	36.8
Average	43.9	56.1	64.1	35.9	40.5	59.5	56.2	43.8
A Fifth Grade.								
Second set	37.2	62.8	50.0	50.0	41.2	58.8	60.8	39.2
Fourth set	47.3	52.7	48.4	51.6	43.3	56.7	62.4	37.6
Average	42.3	57.7	49.2	50.8	42.3	57.7	61.6	38.4
B Fifth Grade.								
Second set	38.8	61.2	62.7	37.3	40.7	59.3	57.7	42.3
Fourth set	42.7	57.3	50.4	49.6	36.5	63.5	55.6	44.4
Average	40.7	59.3	56.5	43.5	38.6	61.4	56.6	43.4
A Sixth Grade.								
Second set	46.9	53.1	65.2	34.8	43.3	56.7	58.9	41.1
Fourth set	42.9	57.1	69.6	30.4	51.7	48.3	57.5	42.5
Average	44.9	55.1	67.4	32.6	47.5	52.5	58.2	41.8
B Sixth Grade.								
Second set	41.9	58.1	67.6	32.4	45.6	54.4	79.4	20.6
Fourth set	40.5	59.5	50.6	49.4	41.9	58.1	70.6	29.4
Average	41.2	58.8	59.1	40.9	43.7	56.3	75.0	25.0
A Seventh Grade.								
Second set	41.9	58.1	61.1	38.9	40.7	59.3	45.6	54.4
Fourth set	39.8	60.2	59.8	40.2	38.9	61.1	54.5	45.5
Average	40.8	59.2	60.5	39.5	39.8	60.2	50.1	49.9
B Seventh Grade.								
Second set	45.8	54.2	67.5	32.5	48.1	51.9	55.8	44.2
Fourth set	49.2	50.8	44.2	55.8	41.1	58.9	58.9	41.1
Average	47.5	52.5	55.8	44.2	44.6	55.4	57.4	42.6
Eighth Grade.								
Second set	35.9	64.1	57.5	42.5	41.2	58.8	54.3	45.7
Fourth set	43.1	56.9	70.9	29.1	43.9	56.1	56.7	43.3
Average	39.5	60.5	64.2	35.8	42.5	57.5	55.5	44.5

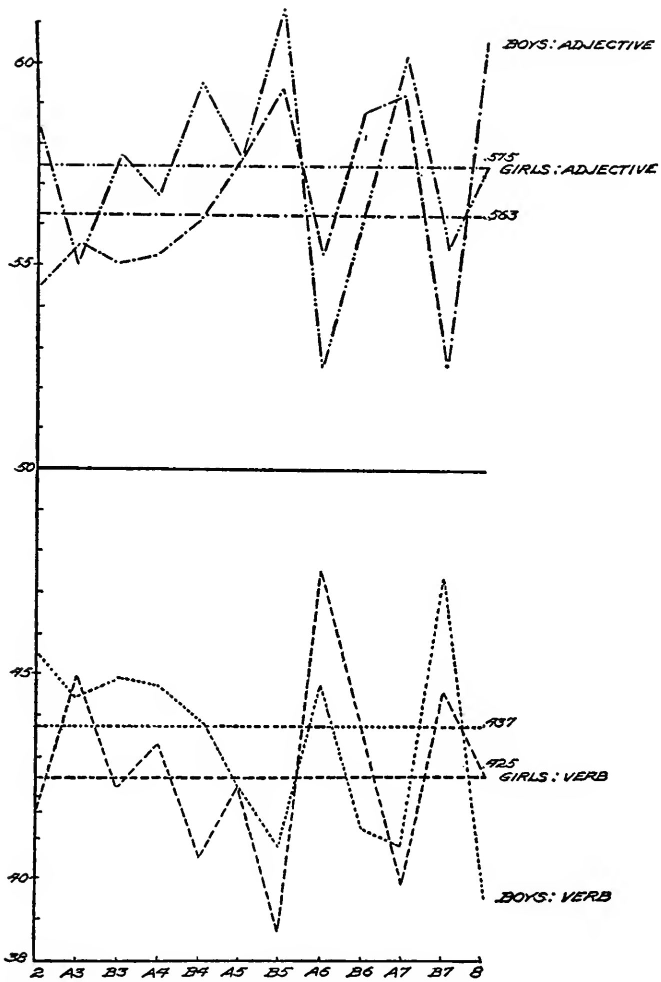

PLATE D. Choice between verbs and adjectives by boys and girls respectively. The ordinates indicate in per cent. the amount of preference shown for each of the two groups of words; the abscissæ, the school-grades. (From Table IV.)

Table II. and Plates B and E show a slight preference among the boys for words denoting activity and not quite so great a preference among the girls for words denoting passivity. The preference for first words is shown in nine grades among the boys and in ten grades among the girls.

Table III. and Plates C and E show a preference for words

pertaining to food among both boys and girls. The preference is more marked among the boys and is shown for all the grades (Plate C); whereas it is shown in ten of the grades among the girls. The greatest difference in the choice of contrasting words is shown here in the result for the boys of the B6 and A7 grades. There is a predominance here of first words in nine grades among the boys and in all grades among the girls.

TABLE V

Showing the total number and percentage of each class of words chosen and also the total number and percentage of first and second words chosen.

	Time	Space	First	Second	Activ.	Passiv.	First	Second
Boys	3206 53.9	2743 46.1	3233 54.3	2716 45.7	2812 51.1	2688 48.9	2976 54.1	2524 45.9
Girls	3458 52.8	3084 47.2	3562 54.5	2980 45.5	3039 49.1	3153 50.9	3310 53.5	2882 46.5
	Dress	**Food**	**First**	**Second**	**Verb**	**Adj.**	**First**	**Second**
Boys	2467 41.9	3409 58.1	3336 56.7	2540 43.3	2408 43.7	3100 56.3	3267 59.3	2241 40.7
Girls	2942 45.4	3544 54.6	3666 56.6	2820 43.4	2584 42.5	3492 57.5	3614 59.5	2462 40.5

Table IV. and Plates D and E show a predominance of adjectives in all the grades among both boys and girls; here the preference is greater among the girls. There is a predominance of first words among the girls in all grades and among the boys in ten grades.

Although Mrs. Manchester's work was upon men and women, nevertheless a comparison of these results with hers is interesting. The element of time was more noticeable among the ideas of men and that of space among the ideas of women. These boys and girls alike show a preference for time ideas—the preference being slightly greater among the boys. The idea of activity was characteristic of the men's lists while that of inactivity appeared in the women's lists. The boys show, on the average, a preference for words denoting activity, although even among them there is in six of the twelve grades a slight predominance of words denoting passivity. Among the girls there is a noticeable preference for words denoting passivity. The men were slightly in advance

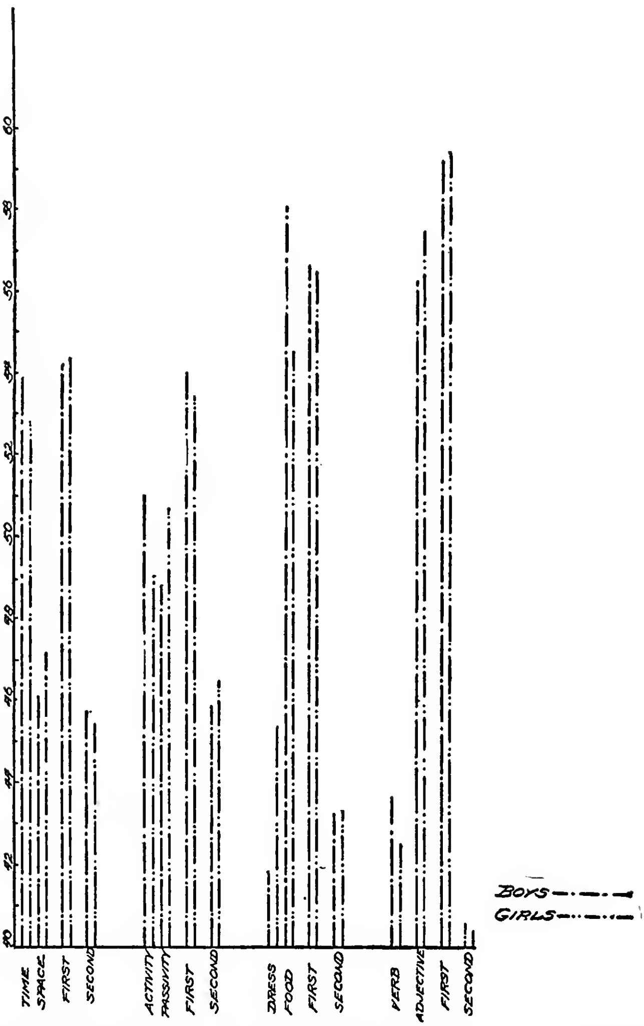

PLATE E. Choice between the various groups of words, and the choice between first and second words when the several groups were offered. (From Table V.)

of the women with reference to food; whereas the women exceeded the men in the class referring to wearing apparel. In this experiment, both boys and girls show a marked preference for words denoting food; the preference is stronger among the boys. The men led in the number of verbs written and the women in the number of adjectives. Both boys and girls show a great preference for adjectives.

The larger problem concerning the difference at various ages in the affective life of boys and girls may be considered in two aspects: (1) What may be inferred from the results of this experiment concerning the problem? (2) How are such inferences related to the conclusions of other investigators?

Considering now the first of these, Plates A to D suggest a tendency toward an increase of preference with age. That this tendency might be more carefully studied, Table VI. was prepared. This table shows in percentages the change of preference based on the different classes of ideas, with age, on the part of the boys and the girls. Table VII. shows the change of preference based on the position of the words, with age, on the part of the boys and the girls. Plate F gives in graphic form the data of Table VI.; and Plate G, the data of Table VII.

Comparing the two plates (F and G), one sees that the preference based upon the position of the words is more pronounced than the preference based upon ideas. This is certainly contrary to the expectations of the writer at the beginning of the experiments. Although one is not surprised to find the pupils in the second and third grades showing a preference for words merely according to their position, one does not look for such purely superficial preference in the upper grades. One's general observation that children grow more thoughtful after nine or ten years of age is upheld by such studies as Mrs. Mary Sheldon Barnes' and Miss Vostrovsky's. Mrs. Barnes[1] has shown that the ability of both boys and girls to make legitimate and critical inferences from an historical incident increases after eight years of age. The increase at first is gradual, then more rapid. Miss Vostrovsky[2] found an increase with age in definite answers to the question, "Why did you select your last book?" She also found an increase of disbelief in superstitions with age.[3]

Comparing Plates F and G more closely, one sees that among the girls there are five grades in which the curve for preference based upon ideas rises above 10 per cent.; in four

[1] 'Studies in Historical Method,' Boston, 1896, p. 68.

[2] 'Study of Children's Superstitions,' Barnes' 'Studies in Education,' Vol. 1, p. 123.

[3] 'Children's Reading Tastes,' *Pedagogical Seminary*, Vol. 6, p. 523.

TABLE VI

SHOWING THE CHANGE OF PREFERENCE, IN PER CENT., WITH GRADE[1]

List of Time and Space Words

(With two exceptions, where a preference for Space is indicated by Sp., the preference is for Time words)

Grade	2	A3	B3	A4	B4	A5	B5	A6	B6	A7	B7	8
Boys	3.6[1]	11.	6.4	10.8	.2	5.	5.	6.	3.8	15.4	6.6	12.
Girls	1. (Sp.)	6.4	3.4	3.8(Sp.)	5.2	3.6	11.	2.6	.6	9.6	25.2	7.
Boys and girls combined	2.3	8.7	4.9	7.3	2.7	4.3	8.	4.3	2.2	12.5	15.9	9.5

List of Words Denoting Acitvity and Passivity

(Preference is for Activity except where Passivity is indicated by P.)

Grade	2	A3	B3	A4	B4	A5	B5	A6	B6	A7	B7	8
Boys	3.6(P)	3.4	4. (P)	1.8	.4(P)	.8(P)	8.4	5.	10.8	9.	3.4(P)	1.4(P)
Girls	6.8	1.	3.2	8. (P)	20.8(P)	1.4(P)	2.8(P)	0.	9.2(P)	4. (P)	12.2(P)	.6
Boys and girls combined	5.2	2.2	3.6	4.9	10.6	1.1	5.6	2.5	10.	6.5	7.8	1.

List of Words Denoting Dress and Food

(Preference is for Food except where Dress is indicated by D.)

Grade	2	A3	B3	A4	B4	A5	B5	A6	B6	A7	B7	8
Boys	.8	.4	3.4	17.	4.8	16.2	12.2	32.6	50.	43.8	13.2	15.4
Girls	11.8	9. (D)	14.4	17.2	7.4	1.4	11.	8.8(D)	11.	38.2	5.8	7.
Boys and girls combined	6.3	4.7	8.9	17.1	6.1	8.8	11.6	20.7	30.5	41.	9.5	11.2

List of Verbs and Adjectives

(The preferences throughout are for Adjectives)

Grade	2	A3	B3	A4	B4	A5	B5	A6	B6	A7	B7	8
Boys	9.	11.2	10.2	10.6	12.2	15.4	18.6	10.2	17.6	18.4	5.	21.
Girls	16.8	10.	15.6	13.4	19.5	15.4	22.8	5.	12.6	20.4	10.8	15.
Boys and girls combined	12.9	10.6	12.9	12.	15.8	15.4	20.7	7.6	15.1	19.4	7.9	18.

Average Course of Preference; all Lists Combined

Grade	2	A3	B3	A4	B4	A5	B5	A6	B6	A7	B7	8
Boys	4.3	6.5	6.	10.1	4.4	9.4	11.1	13.5	20.6	21.7	7.1	12.5
Girls	9.1	6.6	9.2	10.6	13.3	5.5	11.9	4.1	8.4	13.1	13.5	7.4
Boys and girls combined	6.7	6.6	7.6	10.3	8.8	7.4	11.5	8.8	14.5	17.4	10.3	9.9

[1] Table I. shows that for the boys in the second grade the average selection of time words was 51.8 per cent. of the total number of time and space words presented; the average selection of space words was 48.2 per cent. This indicates a preference of 3.6 per cent. in favor of time words.

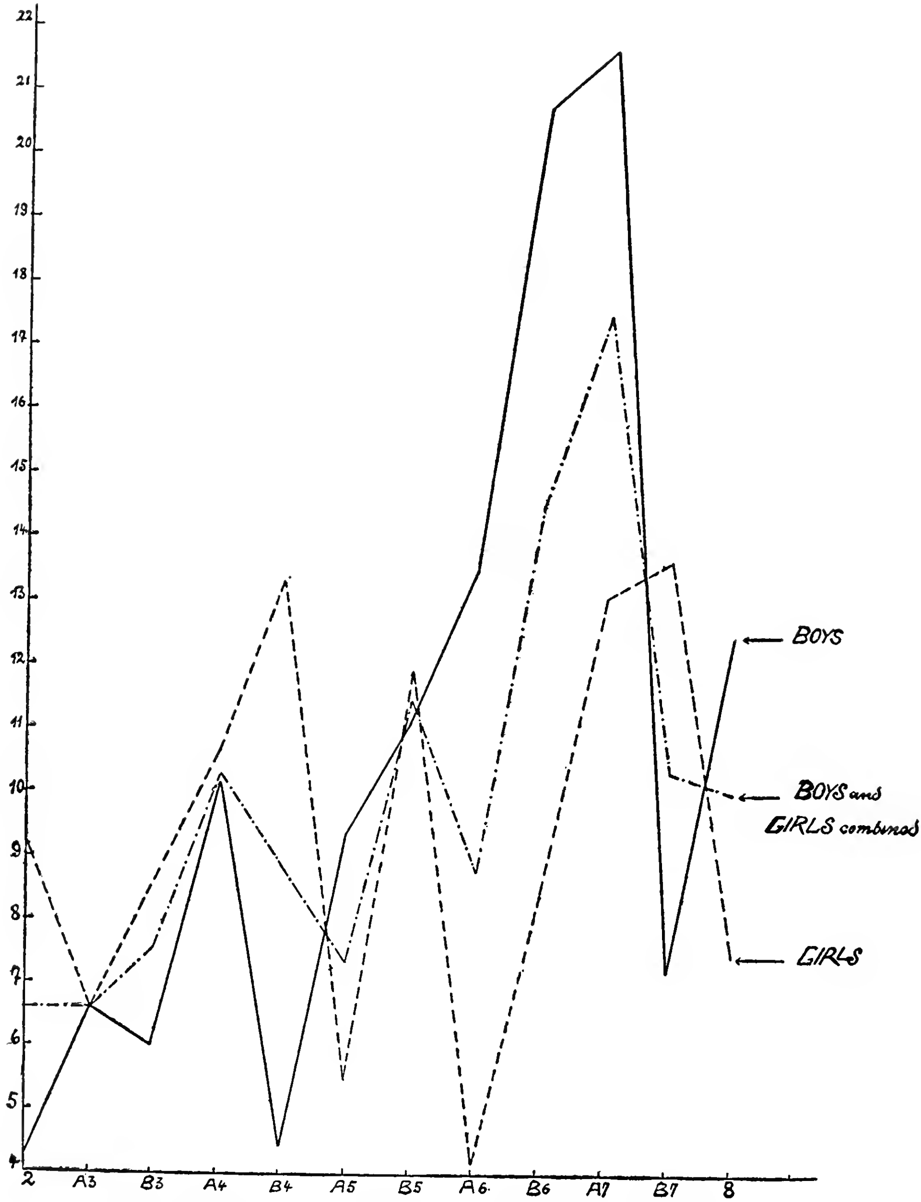

PLATE F. Varying degree of preference arising from difference of the *ideas* conveyed by the words offered. The ordinates indicate the relative strength of such preference; the abscissæ, the school-grades. (From Table VI.)

TABLE VII

SHOWING, IN PERCENTAGES, THE CHANGE OF PREFERENCE FOR A WORD IN A SPECIAL POSITION

The preference is for first words except where the mark (2) appears

Grade	2	A3	B3	A4	B4	A5	B5	A6	B6	A7	B7	8
List of Time and Space Words												
Boys	3.2(2)	16.8	6.8	9.6	27.8	3.6(2)	1.4	16.2	8.8	9.	12.8	22.2
Girls	12.6	26.8	6.4	4.8	20.4(2)	.2	3.6	21.8	42.2	16.2	11.	16.6
Boys and girls combined	7.9	21.8	6.6	7.2	24.1	1.9	2.5	19.	25.5	12.6	11.9	19.4
List of Words Denoting Activity and Passivity												
Boys	13. (2)	12.	8.6	5.8	28.	2. (2)	3.2(2)	17.6	19.6	12.	22.8	23.2
Girls	3.2	20.6	3.4	2.	2.8(2)	2.8	.6	4.4(2)	46.4	13.	9.8	23.2
Boys and girls combined	8.1	16.3	6.	3.9	15.4	2.4	3.8	11.	33.	12.5	16.3	23.2
List of Words Denoting Dress and Food												
Boys	.2(2)	13.8	18.2	16.2	18.4	3.2(2)	19.2	29.6	22.6	9.8	6.8(2)	28.8
Girls	7.8	24.8	11.	3.6	2.4	10.2	9.4	35.	45.	1.2	7.2	12.6
Boys and girls combined	4.	19.3	14.6	9.9	10.4	6.7	14.3	32.3	33.8	5.5	7.	20.7
List of Verbs and Adjectives												
Boys	.6(2)	25.	36.6	3.8	28.2	1.6(2)	13.	34.8	18.2	21.	11.6	28.4
Girls	13.8	42.6	21.	12.6	12.4	23.2	13.2	16.4	50.	.2	14.8	11.
Boys and girls combined	7.2	33.8	28.8	8.2	20.3	12.4	13.1	25.6	34.1	10.6	13.2	19.7
Average Course of Preference for a Word in a Special Position in all Lists Combined												
Boys	4.3	16.9	17.6	8.8	25.6	2.6	9.2	24.6	17.3	12.9	13.5	25.7
Girls	9.3	28.7	10.4	5.8	9.5	9.1	6.7	19.4	45.9	7.7	10.7	15.8
Boys and girls combined	6.8	22.8	14.	7.3	17.6	5.9	8.	22.	31.6	10.3	12.1	20.8

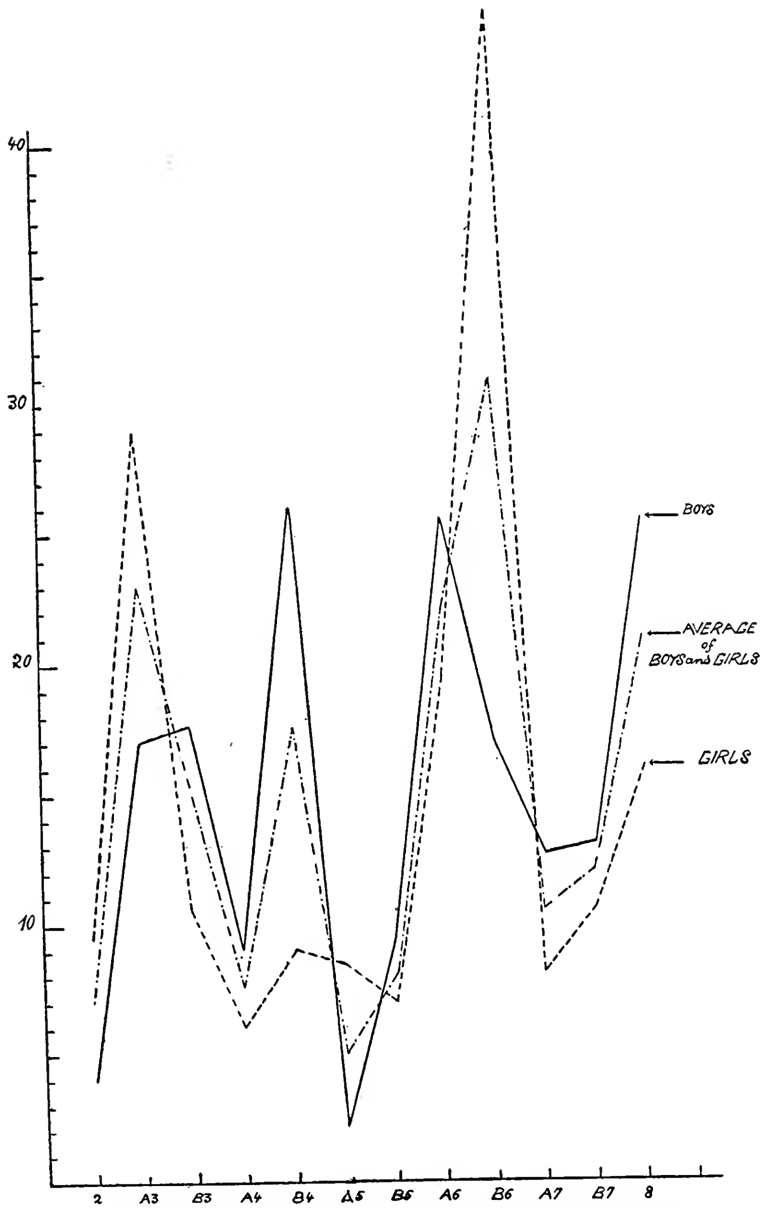

PLATE G. Varying degree of preference arising from mere *position of the word in its pair*. The ordinates indicate the relative strength of such preference; the abscissæ, the school-grades. (From Table VII).

of these grades the curve for preference based upon the order of words is below 10 per cent. There are six grades, in which the curve for preference based upon the order of words is above 10 per cent.; in five of these grades the preference based upon ideas is below 10 per cent. Among the boys, there are six grades in which the curve for preference based upon ideas rises above 10 per cent.; in four of these grades, the curve for preference based upon the order of the words rises above 10 per cent. There are eight grades in which the curve for preference based upon the order of the words rises above 10 per cent.; in five of these grades, the curve for preference based upon ideas rises above ten per cent.

Table VIII

Showing the Degree of Intensity of Preference, in per cent. Computed from Table V which Shows the Average Preference for the Twelve Grades

Time and space words.
- boys 7.8 in favor of time.
- girls 5.6 in favor of time.

Words denoting activity and passivity.
- boys 2.2 in favor of activity.
- girls 1.8 in favor of passivity.

Words relating to dress and to food.
- boys 16.2 in favor of food.
- girls 9.2 in favor of food.

Verbs and adjectives.
- boys 12.6 in favor of adjectives.
- girls 15. in favor of adjectives.

Average preference for first words in all classes of words.
- boys 12.2 in favor of first words.
- girls 12.05 in favor of first words.

With the double check in the arrangement of the words, described on page 348, one would expect that where the preference for first words is pronounced, there would be a diminished preference for either class of ideas, and vice versa. And yet, to show that the preference for first words may be most apparent in connection with a preference for the idea conveyed by the word, a copy of two lists of words with actual elections is submitted. The A sixth grade boys' lists for words relating to dress and food are selected because Table III shows a marked preference for words relating to food and also for first words.

As was stated previously, these two sets were written some five weeks apart. The number after each word indicates the number of boys who wrote that word. First words are selected 215 times in the 'second set'; of these, 125 relate to food, 90 relate to dress. In the 'fourth set,' first words are selected 238 times; of these, 157 relate to food, 81 relate to dress.

Table B

A-Sixth Grade: Boys' Elections of Words Relating to Dress and Food

Second Set		Fourth Set		Second Set		Fourth Set	
dress	13	grapes	16	collar	8	cheese	15
grapes	4	dress	2	cheese	9	collar	3
nut	12	cap	11	banana	12	ribbon	6
cap	5	nut	7	ribbon	5	banana	12
hat	11	pie	14	velvet	7	apple	17
pie	6	hat	4	apple	10	velvet	1
berries	12	gloves	10	supper	14	cloak	6
gloves	5	berries	8	cloak	3	supper	12
coat	11	cake	16	silk	9	food	14
cake	6	coat	2	food	8	silk	4
bread	12	skirt	9	beef	12	lace	7
skirt	5	bread	9	lace	5	beef	11
vest	8	meat	16	tailor	9	turkey	17
meat	9	vest	2	turkey	8	tailor	1
fruit	11	shoes	9	pudding	14	necktie	8
shoes	5	fruit	9	necktie	3	pudding	10
stockings	8	potato	15	button	7	carrot	17
potato	9	stockings	3	carrot	10	button	1
butter	12	woolen	9	dinner	14	shawl	6
woolen	5	butter	9	shawl	3	dinner	12

Since other tabulations show the same results as the one submitted, one may conclude that the first word was given the greater preference when it contained the more attractive idea; when the more attractive idea was second, the preference for the first word either was much reduced or disappeared entirely. Although with the majority of pupils it would appear that the two different kinds of preference—the one based on idea, the other based on mere order of presentation—tended now to reinforce and now to offset each other, yet with certain individuals this is not true; they let themselves be influenced solely by position, writing the first word (or the second word) of the entire list of twenty words. Indeed, several wrote the

first (or second) word of more than one list. The following tabulation throws some light on the influence of this factor in the different grades.

Table C

Showing Selection of Words According to Position Exclusively

	Boys		Girls	
Second Grade.				
First word	5 boys	9 lists	5 girls	16 lists
Second word	7 boys	11 lists	5 girls	8 lists
A Third Grade				
First word	5 boys	10 lists	7 girls	9 lists
B Third Grade				
First word	1 boy	2 lists	1 girl	1 list
Second word	2 boys	4 lists	1 girl	2 lists
A Fourth Grade.				
First word	3 boys	4 lists	1 girl	1 list
Second word			1 girl	1 list
B Fourth Grade.				
First word	4 boys	8 lists		
Second word			1 girl	1 list
A Fifth Grade.				
First word	2 boys	2 lists	2 girls	2 lists
Second word	1 boy	2 lists		
B Fifth Grade.				
First word			1 girl	2 lists
A Sixth Grade.				
First word	7 boys	17 lists	7 girls	10 lists
Second word			5 girls	8 lists
B Sixth Grade.				
First word	1 boy	2 lists	4 girls	13 lists
Second word			1 girl	2 lists
A Seventh Grade.				
First word	2 boys	4 lists	3 girls	6 lists
B Seventh Grade.				
First word	1 boy	2 lists		
Eighth Grade.				
First word	1 boy	2 lists	6 girls	12 lists
Second word			3 girls	8 lists
Total	42 boys	79 lists	54 girls	102 lists

Table C attracts one's attention to three points:

1. The influence of the factor of the position of the word is more frequent in the lowest two grades, as is to be expected. The excessive breaking out in the A sixth grade is puzzling; yet among the girls, the preference for the first word and for the second word tend to balance. In the eighth grade, one might suspect a conspiracy among the girls, since nine out of ten girls are influenced by the order of presentation. However, it seems improbable that any plan of writing the words

could have been prearranged, as the pupils did not know when the words were to be presented. In fact, they were rather led to think that each presentation was the last.

2. Table C shows that a certain order was followed in one hundred and two lists among the girls and in seventy-nine lists among the boys—giving this factor a greater frequency among the girls in the ratio of 10 to 8. This frequency among the girls is still more pronounced, even when one allows for the greater number of girls' papers. There were 648 girls' lists of twenty words and 600 boys' lists of twenty words—or a ratio of about 16 to 15.

3. This tabulation runs parallel at certain points with the general preference based on the position of the words shown in Table VII. In Table C, in the second grade, the preferences for the first word and for the second word are almost balanced among boys and girls. In Table VII. there is a comparatively small percentage of preference for the position of the word shown in this grade. In the A and B fifth grades, Table C shows little influence of this factor of the position of the word; in Table VII. the percentage of preference is low. In the A sixth grade, one would expect the preference for the first word to more or less balance the preference for the second word among the girls (Table C). But Table VII. shows a marked preference for first words. However, among the boys in that grade the two tables are parallel. The girls of the eighth grade are greatly influenced by this factor of the position of the words (Table C), and there is also a marked preference shown in Table VII. Yet Table VII. gives a high percentage of preference for the position of a word for the boys of the eighth grade while Table C shows almost no influence of this factor. However, the small number of boys in this grade must be considered.

With reference to the first aspect of the larger problem; namely, what inferences may be drawn from this experiment concerning the differences in the affective life of boys and girls of various ages, the results warrant the statement that there is with age, a general, though very irregular, increase in the preference shown for the various classes of ideas.

Furthermore, the preference is more marked among the boys than among the girls. The irregularities of the curves of Plate F may be due to several reasons. At least eight nationalities are represented in these children, and in some of the homes the parents do not speak English. And with a greater number of children the curve would tend to be more regular; 150 boys and 162 girls are the basis for these curves.

It is also noticeable that in some of the grades the children are over age. Ayers[1] gives the following as the normal age:

Grade.	2	3	4	5	6	7	8
Age.	7–9	8–10	9–11	10–12	11–13	12–14	13–15

The following (to repeat from Table A, p. 348) are the average ages of the pupils in the present experiment:

Grade	2	A3	B3	A4	B4	A5
Age of boys.	9.3	10.1	10.9	10.9	12.4	11.3
Age of girls.	8.6	9.3	9.3	11.	11.7	12.1
Grade	**B5**	**A6**	**B6**	**A7**	**B7**	**8**
Age of boys.	13.9	13.3	13.6	13.6	14.5	15.1
Age of girls.	12.1	12.9	14.	12.9	14.	14.8

The table for the normal age is given for the grade; *i. e.*, for the sixth grade, the normal age is from 11 to 13. Any pupil in the sixth grade who is over 13 years of age is beyond the normal age. Or, any pupil in the A sixth grade who is over 12 years of age is beyond the normal age. Glancing at Table A, one finds that the boys in the B3, B4, B5, A6, B6 and B7 grades are beyond the normal age; the girls in the A3, A4, B4, A5, A6 and B6 grades are beyond the normal age.

After studying the irregularities in the curves in Plate F, one finds that in the grades in which the drops occur the pupils are over age. In the boys' curve, the points are the B3, B4 and B7 grades; in the girls' curve, the A3, A5 and A6 grades. Yet, the boys of the B5, A6 and B6 grades are also above the normal age and at these points the curve is steadily rising. The girls of the A4 and B6 grades are also above the normal age and the curve is rising at these points. In the B4 grade, in which the girls are beyond the normal age, their curve has reached one of the peaks.

[1] 'Laggards in Our Schools,' 1909, p. 38.

Considering the second aspect of the larger problem, these curves would have been more comparable with the curves of other studies if the age of the pupils instead of the grades had been used as the basis for the curves. However, some rough comparisons may be of interest. In a 'Study of Children's Reading Tastes' by Miss Vostrovsky,[1] curves are given which show the increase, with age, in definiteness of answer to the question "Why did you select your last book?" The curves show that the boys increase gradually in definiteness with no drops, whereas the girls' increase is not so great nor so steady. There are two drops in the girls' curve—at ten and at fifteen years of age—and a sudden rise from fourteen to fifteen. She infers from the results that boys are more independent in their selection of books than are girls.

In a 'Study of Children's Superstitions'[2] by the same writer, a growth in the critical spirit as children become older is shown. The curves showing the number of superstitions described as untrue by boys and by girls display the same differences between boys and girls as was mentioned in her other study. About the same general difference between boys and girls is shown in Plate F as was shown in Miss Vostrovsky's studies.

In Donaldson's[3] showing of the variation in brain weight during the first twenty-five years, the curves for both boys and girls are far more regular than in my Plate F, and yet there are some points of similarity. From eleven to thirteen years of age, the boys' curves show a steady rise both in the preferences here studied and also in brain weight; then a drop in both to fourteen and then a rise in both to fifteen. Among the girls, there is a decided rise in both from thirteen to fourteen, then a drop in both. Fourteen years is the highest point in both. There is far less similarity between these curves of Plate F and the two curves which Donaldson[4] gives of the changes (A) in the length of the head and (B) in the breadth of the head.

[1] *Pedagogical Seminary*, Vol. 6, p. 523.

[2] In Barnes' 'Studies in Education,' Vol. 1, p. 123.

[3] 'Growth of the Brain,' 1895, p. 105.

[4] 'Growth of the Brain,' 1895, p. 112.

Important curves for comparison are those given by Burk in a study on the 'Growth of Children in Height and Weight.'[1] Two of these sets of curves give the annual percentage of increase in weight (I. J.) and the annual percentage of increase in height (K. L.) of the average American girl and boy. There are several points of similarity between his curves I. J. and my own F. The girls' curves drop from eight to nine years in both; and twelve years, the highest point in I. J., is one of the three peaks in F. However, there is a steady rise from nine to twelve in I. J.; whereas in F. there is a drop and then a rise before twelve years is reached. The boys' curves I. J. and K. L. are similar from nine to fifteen years. There is a drop from ten to eleven in these, comparable to that from A3 (10 yrs.) to B3 (11 yrs.) in F. But, in the curves I. J. and K. L., there is a steady rise from eleven to fifteen, the highest point; whereas in F, there is a drop from A4 (11 yrs.) to B4 (12 yrs.) followed by a rise to A7 (13.6 yrs.), the highest point; then another drop and another rise.

It is thus probable that there is some connection between general physical and mental growth and the development of the affective life, of which preference is an aspect.

[1] *The American Journal of Psychology*, Vol. 9 (1897-98), p. 263.